The American Political Science Review

Articles and Essays by William H. Riker and Steven J. Brams, Robert Axelrod, Vernon Van Dyke, Harry W. Blair, Lester M. Salamon and Stephen Van Evera, Sam Kernell, A. James Gregor

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Harry Eckstein
Authority Patterns: A Structural Basis for Political Inquiry

Douglas D. Rose, Virginia Gray, Jack L. Walker
Innovation in State Politics

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Economic Growth and Intergenerational Change in Japan

William Zimmerman
Issue Area and Foreign-Policy Process

Warren Lee Kostroski
Party and Incumbency in Postwar Senate Elections

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POLITICAL SCIENCE

Announcing:

AN INTRODUCTION TO AMERICAN GOVERNMENT

Kenneth Prewitt and

Sidney Verba

Written for the beginning student in American government, this text opens with an innovative discussion on the U.S. economic system and how it has shaped the kind of democracy we have. After detailing the origins and development of American "political capitalism," the authors consider its implications on the class structure of America. Other unique questions are considered: The degree to which the principle of democracy in fact has its roots in the hearts and minds of the American people and how the American citizen participates in political life. The authors also discuss how policy is made and who makes it. This discussion is applied to four policy issues: tax policy, medical care, the desegregation of public schools, and Vietnam.

Each chapter concludes with a "controversial question" designed to help the student understand the underlying issues and to see how political differences derive from disputes over the evidence and from differences of political values. The book presents several short sections describing how political scientists study politics, including a section on sample surveys and how they are conducted and a section showing how the student can investigate public policy and study government institutions.

Tentative: 608 pp. $9.95; January, 1974. Instructor's Manual.

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ARTICLES

1142 **Authority Patterns: A Structural Basis for Political Inquiry.** The paper proposes a novel (even if not wholly unprecedented) solution to an old and fundamental problem: What should be the scope of political studies? Arguments have long been directed against the conventional equation of the scope of the field with the study of "state-organizations" and structures that directly impinge upon such organizations. The arguments are convincing. However, the principal proposed alternatives have important flaws of their own. These alternatives are the extension of the scope of the field to phenomena "functionally" similar to state-organizations and the inclusion in polititcal study of all "asymmetrical" social relations—power, influence, or control relations. By means of the classificatory method of "progressive differentiation," an alternative that seems preferable is worked out: equating political study with the study of authority patterns in any and all social units. That conception of the subject matter of the field, it is argued, avoids all the difficulties raised by other conceptions and affords all of their advantages. Above all, it reconciles subjective interests with scientific (or disciplinary) imperatives and achieves a proper trade-off between the numerousness and homogeneity of phenomena covered by the field—a trade-off critical for the achievement of general, testable, informative empirical theory. A concluding section discusses the place in political study, thus conceived, of the study of international relations and of recent work in "political economy," which appears to focus on symmetrical, not asymmetrical, interactions.

By HARRY ECKSTEIN, IBM Professor of Foreign and International Studies, Princeton University.

1162 **National and Local Forces in State Politics: The Implications of Multi-Level Policy Analysis.** Studies of American states and their policies are severely handicapped by the use of a single level of analysis. The operations of American politics and the assumptions of correlation methodology imply that only a multi-level approach can adequately comprehend state politics. All major aspects of politics, but particularly policies, are distorted by the single level approach. The extent of distortion suggests that relations among states and between states and the national government are the prime determinants of state politics and that the study of states ought to be organized around these relations. This approach accounts for the salient characteristics of state politics, indicating that states are not political systems but collections of tangentially related components of a national system. States which appear systematic derive their coherence from interactions with the national pattern, a process with important residual effects on the legitimacy of state governments.

By DOUGLAS D. ROSE, Assistant Professor of Political Science, Tulane University.

1174 **Innovation in the States: A Diffusion Study.** This study focuses on a nonmonetary dimension of public policy—innovation by states in the fields of education, welfare, and civil rights. Innovation is considered equivalent to the adoption of a law by a state. From the literature on diffusion (or spread) of innovations, the explanation of user interaction is taken, and a simple model with an interaction term is constructed. The model performs fairly well when evaluated by several common criteria. The results do vary somewhat from one issue area to another; other types of supplementary analysis also indicate variation in diffusion patterns according to the issue involved. Political and economic differences among states are found to account for differences in time of adoption, and "innovativeness" is shown to be an issue- and time-specific factor.

By VIRGINIA GRAY, Assistant Professor of Political Science, University of Minnesota.
Comment. By JACK L. WALKER, Associate Professor of Political Science, University of Michigan.
Rejoinder. By VIRGINIA GRAY.

1194 **Economic Growth and Intergenerational Change in Japan.** This article seeks to apply the hypotheses and findings presented by Ronald Inglehart in his "The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies," to the Japanese scene. Inglehart argued that affluence had produced changes in value priorities in Western Europe: the older generation has acquisitive values, while the younger age groups have postbourgeois values. An analysis of Japanese data collected by the Institute of Mathematical Statistics in four national surveys in 1953, 1958, 1963, and 1968 suggests that value changes in Japan produced by rapid economic growth may be somewhat different. Three types of change were found, namely intergenerational, life-cycle, and adult. Compared to its elders, the younger generation is less acquisitive, more democratic, and somewhat more inclined to value freedom. The most important value change, however, appears to be intergenerational change from collectivity orientation toward individuation. It is suggested that individuation has led to a tendency toward privatization and less concern for the electoral process on the part of youth. Whether or not this is a temporary phenomenon is not clear.

By NOBUTAKA IKE, Professor of Political Science, Stanford University.

1204 **Issue Area and Foreign-Policy Process: A Research Note in Search of a General Theory.** The purpose of this research note is to suggest a potential general paradigm for the study of foreign-policy processes. It is explicitly synthetic in that it combines Arnold Wolfers's notion of a continuum, the extremities of which he labels the pole of power and pole of indifference, with Theodore Lowi's efforts to affirm the nexus between issue and policy process. Two questions prove crucial in the determination of issue area: Is or is not the domestic impact of the issue symmetrical? And are the political goods at stake exclusively tangible or not? With the answers to these questions it becomes possible to specify the issue area (distribution, regulation, "interaction-protection," redistribution) in which an event may be classified and to hypothesize the nature of the policy process (the identity of the major actors, the intensity of conflict) to be observed. Particular attention is paid to limited war as a redistributive issue area in order to make the case that redistribution, contrary to Lowi's view, is an important foreign policy process. Finally an effort is made to suggest how issue-based propositions could be utilized in the transnational comparison of foreign policy processes. It is suggested that differences in the policy process across issue areas within a given state may be as great as differences in process within a particular arena of power for two states as different in political system as the United States and the USSR.

By WILLIAM ZIMMERMAN, Associate Professor of Political Science, University of Michigan.

1213 **Party and Incumbency in Postwar Senate Elections: Trends, Patterns, and Models.** Senators who seek re-election usually achieve it. Yet it is not clear whether they tend to win mainly because they are incumbents or because their party is strong in their states. Thus, the two principal questions motivating this study are the following: (1) What is the relative importance of party and of incumbency in influencing the outcomes of Senate elections? (2) How has their relative importance changed over the last quarter-century? To answer these questions, a theory is developed accounting for the outcomes of Senate elections in terms of the major potential sources of electoral support given to the candidates, namely, party loyalty, incumbency, "national tides," and idiosyncratic factors (e.g., issues, personality, local conditions). The theory is then represented in a formal model for which are generated multiple regression estimates of the respective roles of party and incumbency in all postwar Senate contests.

The major finding is that the relative importance of party and incumbency has changed dramatically over the last quarter-century. Party has undergone an overall decline in influence, while incumbency has experienced a roughly proportionate increase. At the same time, the importance of idiosyncratic factors has grown. The implications of these results for broader theories of American politics, including the argument that the United States has been experiencing a "critical realignment," are noted.

By WARREN LEE KOSTROSKI, Assistant Professor of Political Science, Wittenberg University.

1235 **The Paradox of Vote Trading.** Although, conventionally, vote trading in legislatures has been condemned as socially undesirable by both scholars and lay citizens, a recently popular school of scholarship has argued that vote trading improves the traders' welfare in the direction of Pareto-optimal allocations. This essay is an attempt to reconcile the disagreement by showing formally that vote trading does improve the position of the traders but that at the same time trading may impose an external cost on nontraders. In sum, it turns out that sporadic and occasional trading is probably socially beneficial but that systematic trading may engender a paradox of vote trading. This paradox has the property that, while trading is immediately advantageous for the traders, still, when everybody trades, everybody is worse off. Furthermore, vote trading may not produce a stable equilibrium that is Pareto-optimal either for individual members or for coalitions of members.

By WILLIAM H. RIKER, Professor of Political Science, University of Rochester, and STEVEN J. BRAMS, Associate Professor of Political Science, New York University.

1248 **Schema Theory: An Information Processing Model of Perception and Cognition.** The world is complex, and yet people are able to make some sense out of it. This paper offers an information-processing model to describe this aspect of perception and cognition. The model assumes that a person receives information which is less than perfect in terms of its completeness, its accuracy, and its reliability. The model provides a dynamic description of how a person evaluates this kind of information about a case, how he selects one of his pre-existing patterns (called schemata) with which to interpret the case, and how he uses the interpretation to modify and extend his beliefs about the case. It also describes how this process allows the person to make the internal adjustments which will serve as feedback for the interpretation of future information. A wide variety of evidence from experimental and social psychology is cited to support the decisions which went into constructing the separate parts of the schema theory, and further evidence is cited supporting the theory's system-level predictions. Since the schema theory allows for (but does not assume) the optimization of its parameters, it is also used as a framework for a normative analysis of the selection of schemata. Finally, a few illustrations from international relations and especially foreign-policy formation show that this model of how people make sense out of a complex world can be directly relevant to the study of important political processes.

By Robert Axelrod, Assistant Professor of Political Science, University of California, Berkeley.

1267 **Human Rights Without Discrimination.** This paper focuses on the requirement of the U. N. Charter that members shall promote human rights "without distinction as to race, sex, language or religion." It asks: (1) whether "without distinction" means "without any differentiation" or "without discrimination"; (2) whether the naming of race, sex, language, and religion means that "distinction" on other bases remains permissible, or whether what is ruled out is discrimination *of any kind* that affects human rights; and (3) whether the search for equality has not been internationalized, at least in principle, and what the implications of the internationalization may be. The inquiry can be classified as legal/philosophical.

The third question leads to an analysis of the "without distinction" clause of the Charter in terms of the principle of equality. Claims to nondiscrimination are negative claims to equality, and they have as their counterpart claims to affirmative action on behalf of equality—often for groups. The problem is that affirmative action promoting equality for groups entails differentiations among individuals that may be discriminatory. To resolve the dilemma, the test of reasonableness is endorsed—and the search for standards for judging it goes on—at both the domestic and international levels.

By Vernon Van Dyke, Professor of Political Science, University of Iowa.

1275 **Minority Electoral Politics in a North Indian State: Aggregate Data Analysis and the Muslim Community in Bihar, 1952–1972.** Frequent elections and a long tradition of census taking in India should combine to provide excellent scope for aggregate data analysis, but so far they have not, largely because the electoral constituencies and the census tracts do not match. A number of ways have been devised to surmount the problem, none of them very satisfactory. This paper offers a new solution in the form of isoplethic mapping, a method that avoids the shortcomings of other approaches and permits use of demographic and voting data at the level of the state legislative assembly constituency.

Substantively the paper traces patterns of voting for Muslim candidates to the Bihar Legislative Assembly and the relationship between Muslim population distribution and vote polled by different political parties over six elections. Instead of becoming more integrated over time within the general body politic, it appears that the Muslim minority group has become more politically cohesive and better able to elect Muslims to office where their numbers are strong. At the same time, Muslims have become less able to win elections where they are fewer in numbers. This tendency has not reached a state of political polarization between the Hindu and Muslim communities, however.

By Harry W. Blair, Assistant Professor of Political Science, Bucknell University.

1288 **Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation.** Students of political participation have generally taken as given that nonparticipation in politics is a result of apathy, and that apathy is a function of low income, low education, and low status. This article suggests that there are two additional potential explanations of political participation rates besides that offered by the conventional wisdom. One of these acknowledges that political participation for some people in some circumstances involves considerable risk, so that nonparticipation can be explained more accurately in terms of *fear* than in terms of apathy. The other views political participation as a response to a sense of "relative deprivation" or discrimination. After each of these three "models" of political participation is translated into operational terms, it is tested by determining how well it accounts for the variations in black political participation rates in Mississippi during the first half-decade following the 1965 Voting Rights Act. The conclusion that emerges from these tests is that political scientists have erred seriously by overlooking the role of fear in political life. In situations like those faced by blacks in Mississippi, situations that are probably similar to those in parts of the "developing world," apathy compares poorly with fear as an explanation of political participation.

By Lester M. Salamon, Assistant Professor of Political Science and Assistant Professor of Policy Sciences and Public Affairs, Duke University, and Stephen Van Evera, Graduate Student in Political Science, University of California, Berkeley.
Comment. By Sam Kernell, Instructor in Political Science, University of Minnesota.
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By A. JAMES GREGOR, Professor of Political Science, University of California, Berkeley.

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Address correspondence about contributions to the *Review to* Nelson W. Polsby, Department of Political Science, University of California, Berkeley, Berkeley, California 94720. Each manuscript should be accompanied by an abstract of up to 150 words briefly describing the article's contents. All manuscripts and abstracts should be submitted IN DUPLICATE. They should be double-spaced and may be in typed, mimeographed, hectographed, or other legible form. Footnotes should appear at the end of the manuscript, not at the bottom of the page, and should also be doubled-spaced. Manuscripts that do not follow this format will be returned to the authors for retyping.

Since manuscripts are sent out anonymously for editorial evaluation, the author's name and affiliations should appear only on a separate covering page. All footnotes identifying the author should also appear on a separate page.

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EDITORIAL COMMENT

Behind the Bamboo Curtain. One of our most valued colleagues sent us the following memorandum recently. Since it bears centrally upon the intellectual mission of the *Review,* we reprint it herewith:

> On the off-chance that you have not already done a similar analysis yourself, I enclose an exquisitely rigorous tally of the area-related pieces which have appeared in our journal over the past five years. There are a number of tidbits of information that could be teased out of these data, but I particularly call your attention to the final entry.

Area-related articles in APSR, 1968–1972

Western Europe	11
Africa	7
U.S.S.R.	7
Latin America	6
South and Southeast Asia	4
Japan	3
Canada	2
Near East and North Africa	1
Eastern Europe	1
Underdeveloped areas in general	4
Communist countries in general	2
Sino-Soviet relations	1
China	0

> Over the last year or so I have had occasion to satisfy myself that one possible explanation for that entry is false: it is *not* the case that the quality or general interest of scholarship on China is so low as to disqualify it from appearance in our journal. My alternative explanation is that scholarship mirrors reality—that China scholars have lived in splendid isolation, only occasionally interested in those of us who live outside the Middle Kingdom, but also that we have not encouraged them to be outward-looking.
>
> My message to you as our Leader is simple: go to Peking.
>
> Or failing that, go to some of the gurus of Chinese scholarship and invite them to begin submitting stuff to us.
>
> Bring us together.

Disregard Previous Fortune Cookie. This same correspondent then sent us another letter:

> Here is the copy I promised you of those data on area-related articles in journals other than the *APSR.* These data are consistent with those from the *APSR.* Indeed, if you ignore *World Politics,*

Area	J.P.	C.P.S.	C.P.	W.P.	MWJPS	W.P.Q.	Pol.	Totals
Western Europe	14	13	35	12	6	19	8	107
Africa	3	4	7	5	1	4	0	24
USSR	5	1	3	7	1	5	2	24
Latin America	7	8	4	8	4	13	1	45
South and Southeast Asia	1	1	7	5	4	6	3	27
Japan	3	2	3	0	0	3	1	12
Canada	2	0	0	1	0	7	1	11
Near East and North Africa	2	4	4	4	1	3	0	18
Eastern Europe	3	2	3	1	1	1	0	11
Underdeveloped areas	0	19	10	11	1	0	3	44
Communist Countries	2	0	0	1	0	1	1	5
Sino-Soviet Relations	1	0	0	0	0	1	0	2
China	0	0	2	5	0	2	0	9
	43	54	78	60	19	65	20	339

Where possible volumes between 1968 and 1972 were consulted.

there have been only four articles on China in political science journals over the last five years.

One possible explanation is simply that less research is being done by political scientists on China. This is part of the explanation, but only part, as I think I can show. Let us use completed dissertations as one index of overall research activity, both in a direct sense (dissertations become journal articles) and in an indirect sense (dissertations are spawned by productive instructors). Assume a year's lag between dissertations and publication; hence look at dissertations produced 1967–1971. Using Steve Blank's data from the Fall, 1972 issue of *PS,* (and combining China and Japan because Steve's data do not break down the category "East Asia"), we get the following results:

	East Asia	Western Europe
Proportion of all area-related *dissertations* which deal with *this* area:	11%	17%
Proportion of all area-related *articles* which deal with *this* area:	6%	32%

The apparent conclusion: Research on Western Europe claims nearly twice its "fair share" of the general political science "publication space" devoted to area-related research, while research on East Asia claims barely half its "fair share," if by "fair share" we mean "proportionate to volume of (dissertation) research." As I said in my earlier letter, a variety of explanations are possible; I think the East Asian half of the problem, at least, deserves attention.

In general, we only occasionally solicit articles for the *Review*. Perhaps this would be a suitable occasion. We have the feeling that the specialized journals in the Asian field are doing such a good job that they have choked off submissions to the *Review* as well as to most other general purpose journals in political science. With a little luck, however, this note might provoke a freshet of submissions here. If so, we pledge ourselves to give each and every submission serious consideration, and not to restrict ourselves to one from row A and one from row B.

Typoes. Although we do a lot of complaining about the complaints we get, every once in a while a complaint comes along that charms even while it chagrins. Here is one we received the other day:

> Let this letter be my official concession of defeat. I fought the good fight but I lost. In my review of Dale Vinyard's book *The Presidency* (*APSR* 67, March 1973, pp. 234–236), illustrating Vinyard's error-prone ways, I pointed to his spelling of McGinniss. Vinyard insisted on omitting the second and final "s". Strangely, so did the secretaries, assorted editors, and printers of the *APSR* book review section. Painstakingly, I added the requisite "s" in my letters and final proof pages. You won. There is McGinnis, the last "s" glaring in its absence, the last word on page 234. Vinyard must be giggling in his cups as I cry in mine.
>
> David L. Paletz
>
> P.S. It is clever of you to have misspelled Vinyard as Vynard on p. 235 first full paragraph, first column.

What to do about typoes? Our peerless proofreader, Jeanne Dritz, ferrets out nearly all of them. Once in a while she misses. Sometimes, our printer inserts new errors while correcting the old, even after the page proof stage. The March 1973 issue, of which Professor Paletz complains, was a rats' nest of errors by everybody, and there wasn't much we could do about it. Except complain.

Articles Accepted for Future Publication

Paul R. Abramson, Michigan State University, "Generational Change in American Electoral Behavior"

Peter H. Aranson, Georgia Institute of Technology, Melvin J. Hinich, and Peter C. Ordeshook. Carnegie-Mellon University, "Election Goals and Strategies: Equivalent and Non-Equivalent Candidate Objectives"

William I. Bacchus, Commission on the Organization of the Government for the Conduct of Foreign Policy, "Diplomacy for the '70s: An Afterview and Appraisal"

Paul Allen Beck, University of Pittsburgh, "Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior"

Robert A. Bernstein and William W. Anthony, Texas A&M University, "The ABM Issue in the Senate, 1968–1970: The Importance of Ideology"

Gordon S. Black, University of Rochester, "Conflict in the Community: A Theory of the Effects of Community Size"

Steven J. Brams, New York University and Morton D. Davis, City College of New York, "The 3/2s Rule in Presidential Campaigning"

Christopher Bruell, Boston College, "Thucydides' View of Athenian Imperialism"

Walter Dean Burnham, Massachusetts Institute of Technology, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate' "

Blair Campbell, University of California, Los Angeles, "Helvétius and the Roots of the 'Closed' Society"

Edward G. Carmines, State University of New York, Buffalo, "The Mediating Influence of State Legislatures on the Interparty Competition-Welfare Expenditures Linkage"

William Cavala, University of California, Berkeley, "Changing the Rules Changes the Outcome: Party Reform and the 1972 California Delegation to the Democratic National Convention"

John Chamberlin, University of Michigan, "Provision of Collective Goods as a Function of Group Size"

Wayne A. Cornelius, Massachusetts Institute of Technology, "Urbanization and Political Demand-Making: Political Participation Among the Migrant Poor in Latin American Cities"

Lawrence C. Dodd, University of Texas, "Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis"

Bruce Bueno de Mesquita, University of Rochester, "Need for Achievement and Competitiveness as Determinants of Party Success in Elections and Coalitions"

Peter Eisinger, University of Wisconsin, Madison, "Racial Differences in Protest Participation"

David J. Elkins, University of British Columbia, "The Measurement of Party Competition in Multi-Party Systems"

John A. Ferejohn and Morris Fiorina, California Institute of Technology, "The Paradox of Not Voting: A Decision Theoretic Analysis"

Ada Finifter, Michigan State University, "The Friendship Group as a Protective Environment for Political Deviants"

Peter C. Fishburn, Pennslyvania State University, "Paradoxes of Voting"

Ronald P. Formisano, Clark University, "Deferential-Participant Politics: U.S. Political Culture, 1789–1840"

Mark Gavre, University of California, Los Angeles, "Hobbes and His Audience: The Dynamics of Theorizing"

Ted Robert Gurr, Northwestern University, "The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science"

Larry B. Hill, University of Oklahoma, "Institutionalization, the Ombudsman, and Bureaucracy"

Ole R. Holsti, University of British Columbia, "The Study of International Politics Makes Strange Bedfellows: Theories of the Radical Right and the Radical Left"

Donald B. Johnson and James R. Gibson, University of Iowa, "The Divisive Primary Revisited: Party Activists in Iowa"

Kenneth Jowitt, University of California, Berkeley, "An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems"

Donald R. Kelley, Monmouth College, "Toward a Model of Soviet Decision Making: A Research Note"

Stanley Kelley, Jr., Princeton University and Thad W. Mirer, University of Wisconsin, Madison, "The Simple Act of Voting"

Eugene B. McGregor, Jr., Indiana University, "Politics and the Career Mobility of Bureaucrats"

Arthur H. Miller, Ohio State University, "Political Issues and Trust in Government: 1964–1970"

James T. Murphy, Wesleyan University, "Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works

Committee Decision Making"

Thomas C. Nowak and Kay A. Snyder, Michigan State University, "Clientelist Politics in the Philippines: Integration or Instability?"

John C. Pierce, Washington State University and Douglas D. Rose, Tulane University, "Non-Attitudes and American Public Opinion: The Examination of a Thesis"

David Ray, Stanford University, "Membership Stability in Three State Legislatures: 1893–1969"

Stephen G. Salkever, Bryn Mawr College, "Virtue, Obligation and Politics"

Kenneth A. Shepsle, Washington University, "On the Size of Winning Coalitions"

Brian Silver, Florida State University, "Social Mobilization and the Russification of Soviet Nationalities"

Arthur G. Stevens, Jr., University of Virginia, Arthur H. Miller, Ohio State University and Thomas E. Mann, American Political Science Association, "Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group"

Peter G. Stillman, Vassar College, "Hegel's Critique of Liberal Rights"

Kent L. Tedin, College of William and Mary, "The Influence of Parents on the Political Attitudes of New Voters"

Timothy A. Tilton, Indiana University, "The Social Origins of Liberal Democracy: The Swedish Case"

Glenn Tinder, University of Massachusetts, Boston, "Beyond Tragedy: The Idea of Civility"

John Wanat, University of Kentucky, "Bases of Budgetary Incrementalism"

Herbert Weisberg, University of Michigan, "Models of Statistical Relationship"

Roger E. Wyman, Rutgers University, "Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture"

Authority Patterns:
A Structural Basis for Political Inquiry*

HARRY ECKSTEIN
Princeton University

Every field of inquiry rests, at its most fundamental level, on an implicit or explicit conception of its "scope"—that is to say, a conception of the nature of its subject matter and a delimitation of that subject from others. To be sure, overlaps and linkages can usually be found among any set of academic fields, particularly among "sister disciplines," like the various social sciences. Nevertheless, prevalent conceptions of the scope of a field generally imply more than just an arbitrary subjective interest in particular phenomena. They also imply that a subject matter so differs from others that it justifies, or indeed requires, considerable autonomy, in the sense of a distinct set of disciplinary tools and products: special descriptive concepts, methods of inquiry, general theoretical perspectives and approaches and empirical generalizations.

Political scientists' conceptions of the scope of their field have, in the past, tended toward two extremes: on one hand, a very narrow conception centered on "state-organizations"; on the other, a very broad one encompassing all asymmetric human relationships. Both conceptions turn out to have important flaws, either as definitions of subjective interests or delineations of a subject matter in terms of disciplinary considerations. Awareness of these flaws accounts for one of the more peculiar facets of contemporary political studies: the remarkably numerous and various attempts by political scientists to define explicitly the nature of what they study[1]—something normally taken for granted, and therefore left implicit, in academic fields. This discussion presents yet one more such attempt, justified by lack of consensus on the matter and failure (as it seems to me) to work out a satisfactory delimitation of the field's scope.

Politics as State-Organizations

Until quite recently, the predominant (for a long time, in fact, fully consensual) conception of political phenomena was, quite simply, that they had to do with "governments," or "state-organizations," as we know them in the modern West, together with organizations subsumed to them, like local governments, or directly impinging on them, like political parties. Most political scientists discerned little need to worry about the precise meaning of terms like government and the state. Hence the prevalent approach was rather like that predominant in contemporary studies of modernization: modernity is what exists in Western Europe and North America. Differences in meaning, mostly implicit, were many,[2] but mainly shades and nuances around a common and familiar core. State-organizations were thought of as those formally organized structures of societies that specialize in the exercise of "sovereignty," as that term has been understood since, roughly, the early seventeenth century: specialized organizations that make laws, implement them, and resolve conflicts arising under them, and have a uniquely "legitimate" right to do so.

One basis for that conception of the scope of politics was certainly the subjective interests of political scientists—what they wanted to know about. Another, made explicit only when the conception was attacked, was that sovereign states and their organizations are *sui generis* among social structures, thus constituting a subject matter best kept separate from others. This argument was based on various grounds: (1) the state's supposedly unique membership structure (the fact that it is not associational, i.e., not something one can join or opt out of at will or low cost); (2) its supposedly unique scope (ability to regulate subsidiary units without being regulated by a superior one); (3) its supposedly unique functions (above all, inte-

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* This paper was written as part of a project financed by the National Science Foundation, to which my thanks is due. The basic idea germinated in the course of a research and teaching project supported by the Advanced Research Projects Agency, in which Ted Robert Gurr was a partner and in which numerous Princeton graduate students participated. All influenced my thinking, and Gurr provided a close critique of a preliminary draft of the paper which was delivered at the 1972 Annual Meeting of the American Political Science Association.

[1] For a list of ten such definitions (a sample, not an inventory) see Harry Eckstein, "The Concept 'Political System,'" Paper presented at the 1963 Annual Meeting of the American Political Science Association.

[2] No fewer than 145 different definitions of the term "state" are claimed to have been identified by Charles H. Titus. See his "A Nomenclature in Political Science," *American Political Science Review*, 25 (February 1931), 45–61.

grating the various groups of society, or mediating among them); or (4) the supposedly unique instrumentality it uses to perform its tasks (legitimate coercion). A certain view of history, or perhaps cultural arrogance, also lay behind the emphasis on the formal organizations of sovereign states. This was the view that sovereign state-organization, as developed in the modern West, was the natural destiny of all societies, and not a remote end but one achieved as soon as a modicum of "civilization" was attained—so that the kinds of polities described by early anthropologists like Maine and Fraser could be dismissed as simply "prepolitical."[3]

It is true that political scientists have long been concerned with all sorts of organizations and activities that impinge upon states but are not, formally, parts of them. However, their concerns with such phenomena initially derived from and were circumscribed by their very identification of politics with state-organizations. Attacks on that identification can be traced back only to about 1930 and became common only in the 'fifties.[4] The chief factor that made the objections common then, and now, involves the broadening geographic interests of political scientists: their new and growing interest in the politics of "developing" societies. In such societies, a search for specialized formal organizations exercising the attributes of sovereignty, and displaying the supposedly unique characteristics of states, sometimes reveals nothing at all or, more commonly, a facade behind which political realities lie rather ineffectively hidden. This is most obvious, of course, in still largely tribal societies, where functionally diffuse structures based on primordial identifications remain the focus of social regulation, whether or not imitation-states (or rather capitals) have been superimposed on them; and between these and modern Western state-organizations runs an extensive continuum on which different approximations to what we know as statehood can be discerned. The narrow traditional notion of political phenomena did not serve subjective interests in such societies, nor prevent the feeling that they had, in some sense, a "political" life like other polities, even at the most primitive levels of social organization.[5]

Doubts about the narrow conventional conception of politics having thus been engendered, it became manifest that the conception was unserviceable even for an extensive interest in the politics of Western societies. The focus on state-organization restricts study to Western societies roughly after the Peace of Westphalia (no exact date can be given, of course). If we go back much farther we find the work of sovereign state-organizations performed by a hodgepodge of diffuse, imperfectly differentiated, more or less autonomous institutions: royal dynasties, ecclesiastical structures, municipal councils, guilds, merchant companies, land-owning nobles, and universities. We find no "constitutions" as formal organizational blueprints, only charters, compacts, bills (and sometimes so-called Constitutions, like those of Clarendon) as contractual agreements or explicit statements of usage among competitive and overlapping *potentias*. We find no monopolies, actual or pretended, of legitimate coercion. We find, instead, a long and slow fashioning of sovereignty and its instruments as a gradual process of "political expropriation"[6] that seems more tied to special historical circumstances than to the universal attributes of civilized life. Yet one has the sense that "politics" antedates that process and went on throughout its unfolding; and political scientists anxious for light on political development in non-Western societies turn, not unnaturally, to the earlier histories of Western polities themselves.

As the boundaries between state-organizations and other structures for carrying on government and politics weakened, another objection against the focus on state-organizations also emerged. This criticism involved a view occasionally advanced earlier (e.g., by Merriam);[7] that, apart from possibly unimportant variables like size, there is not all that much difference between "public" governments and "private" ones—not enough, at any rate, to warrant a generic distinction on which to rest disciplinary divisions and all they entail.[8] This view is quite different from one admitting the study of "private" groups into the field as influences on public power and policy. The latter view treats the groups as related to political systems through boundary-exchange; the former admits them as political systems in their own right. It thus implies that, in excluding private governments, political science deprives itself of much material useful for building empirical theories while, at the same time, failing to make an optimum contribution to the general understanding of social life.[9]

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[3] For a choice example of this view, see William Archibald Dunning, *A History of Political Theories* (New York: Macmillan, 1902).

[4] I am ignoring here certain much earlier normative political philosophers who are discussed below. The reason for ignoring them at this point is stated in the later passages.

[5] See the very large literature on "primitive" political systems: e.g., M. Fortes and E. E. Evans-Pritchard, *African Political Systems* (London: Oxford University Press, 1940); I. Schapera; *Government and Politics in Tribal Societies* (London: Watts, 1956); A. I. Richards, editor, *East African Chiefs* (London: Faber, 1960); and Lucy Mair, *Primitive Government* (Harmondsworth: Penguin Books, 1962).

[6] Max Weber, *From Max Weber: Essays in Sociology* (New York: Oxford, 1946), p. 83.

[7] Charles Merriam, *Public and Private Governments* (New Haven: Yale, 1944).

[8] See especially Robert A. Dahl, "Business and Politics: An Appraisal of Political Science," *American Political Science Review*, 53 (March 1959), 1–35.

The Functional Conception of Politics

The principal response to the difficulties raised by the notion of politics as centered on state-organizations has been a tendency to define politics in terms of certain social functions rather than social structures.[10] The essential purpose of the functional conceptions has been to raise the traditional focus of political study to a somewhat more general level, at which equivalence between sovereign state organizations and other structural modes of carrying on what is sensed to be political activity becomes apparent.

The logic behind the functional conceptions seems impeccable. Political structures differ widely in space and time, so that the identification of politics with any special structural arrangement unduly restricts and handicaps the field. But different structures can perform the same functions (i.e., can be functional "equivalents"). Hence, the field's scope can be broadened and yet remain clearly defined in relation to nonpolitical phenomena if one identifies the functions of state-organizations and then studies whatever performs those functions where such organizations do not exist or are ill-developed.

Neat as this may seem, the functional revision creates large difficulties of its own. The foremost is that the functions of political structures seem hard to identify, as shown by the fact that practically no functional conception of what is "political" coincides exactly with any other. To be sure, we can discern a sort of latent consensus among the conceptions, but it is on a notion of the "political" function that seems, in the final analysis, to identify nothing in particular at all—to make politics into the study of "the whole of social life in all its facets," as Bentley put it. Some people may not object to this. But it is hardly what the functionalists intend; it may impose intolerable burdens in political research; it probably leads to theories so general (because concerned with such a diversity of phenomena) as to be trivial and uninformative; and if politics becomes equated with social life we will no doubt want to carve it into separate fields, as we now carve up social studies, thus returning to where we started.

The function on which the various functional conceptions converge is that of "maintaining social order"—not in the narrow sense of "law and order" but in the very broad sense of assuring the continuity (viability) of society through its harmonious and efficient operation. That function, as functional sociologists like Parsons make clear, is really a large and diverse (indeed all-encompassing) complex of functions. Parsons lists them under four categories, which will do about as well as any: (a) pattern-maintenance (roughly, imparting to members the society's general norms of behavior and special norms pertaining to its special roles, and getting commitment to them); (b) integration (coordinating the elements of social life to make them mutually supportive, or at least not mutually destructive); (c) goal-attainment (defining and achieving social ends and purposes); and (d) adaptation (generating an appropriate level of social resources, distributing them in ways perceived as just, and mobilizing them to the attainment of social goals). It should be apparent that all institutions of society, in one way or another, to one degree or another, are involved in the performance of these functions. It is also evident that all the functions must be performed in all social institutions if these are to be "viable"—a point that functionalists gleefully emphasize rather than conceal.

Functional definitions of politics thus converge on a view that broadens the scope of the field at the cost of virtually eliminating its boundaries altogether. For this reason, they hardly define the political scientist's subjective interests, however much broader now than formerly; and while all social phenomena might be usefully analyzed in terms of the order-maintaining function at a high level of generality, it hardly follows that disciplinary distinctions beneath that level are unimportant. One might note, in this connection, that functional approaches to special subjects, like politics and economics, have been mainly advanced by sociologists in quest of overarching theories,[11] and that such approaches in politics have, over a span of about a decade and a half, hardly gone beyond the initial definitional level of conceptualization.[12]

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[9] For a more extensive critique of the traditional conception of politics, see David Easton, *The Political System* (New York: Knopf, 1953).

[10] Examples are Gabriel A. Almond, "Comparative Political Systems," *Journal of Politics*, 18 (August 1956), 391–409; Almond and James S. Coleman, editors, *The Politics of the Developing Areas* (Princeton: Princeton University Press, 1960); Talcott Parsons, " 'Voting' and the Equilibrium of the American Political System," in *American Voting Behavior*, ed. Eugene Burdick and Arthur J. Brodbeck (Glencoe: Free Press, 1960); and David E. Apter, "A Comparative Method for the Study of Politics," *American Journal of Sociology*, 64 (1958), 221–237.

The obvious way to save the functional alternative from this difficulty is to identify politics with a single "political" function in performing which modern state-organizations specialize. But this also raises large problems. One, as stated, is to identify the function. Two views on this are common (and others are possible or occasionally advanced): Politics is concerned with "goal-attainment" (Parsons), or politics is concerned with "integration" (Almond). That difficulty might be resolved, in the manner of Bourricaud and Spiro, by a synthesis: Politics is concerned with "integrating conflicting views about social goals."[13] However, that would still leave a major research difficulty. Functions are abstractions, not concrete entities. The first task of any political analysis would therefore be to identify the structures—or, worse, aspects of structures—that have something to do with the performance of the functions, which might be all or any in a society. Parsons puts his finger on the intricacy of that task in a rather offhand footnote: "The polity in (my) sense is *not* identical with government, which we interpret to be a complex of *organizations.* Government has other than political functions, and other organizations participate in the polity."[14] No sweat for people like Parsons, who steer clear of empirical work on concrete phenomena. But what does one do when descending from the rarefied air of conceptualization? What aspects of American government, for instance, are, or are not, political? What aspects of American business, trade unions, schools, professions? The critical point is that, on such a basis, we still wind up studying "the whole of social life in all its facets," quite as much as with the broader functional alternative. In one case, we do so because everything is political, in the other because everything has, or might have, political aspects. And the burden this entails is magnified by having to identify the political aspects without structural guidelines to the task—no guidelines other than that they must have something to do with goal-attainment or integration. No one has ever done this or anything like it; and no wonder.

The moral is that functional conceptions of a subject matter are, in their very nature, to be avoided if satisfactory structural ones, directing attention immediately to concrete phenomena, are available. But can one really devise a structural conception of politics that avoids the main shortcomings of the traditional conception of the field—a conception that does not define the subject in narrowly culture-bound terms, nor make too sharp and rigid the distinction between public governments and private ones?

Politics as Asymmetric Relationships

Attempts to redefine the subject matter of politics antedate recent interests in underdeveloped, developing, and historic polities by quite a bit. The earlier attempts (mainly of the 'thirties) were in fact structural, not in the least culture-bound, and virtually oblivious to distinctions between public and private governments. If nothing else, they demonstrate that the functionalist's way of delimiting the field is not the only alternative to the conventional structural conception.

The object of the earlier redefinitions was to make politics a "discipline" rather than to accommodate expanding subjective interests—save perhaps for a still embryonic interest in private groups as political orders in themselves (rather than because of their boundary-exchanges with "polities"). In gist, their object was to make it possible for political science to be scientific in some serious sense of the term, and those who advanced the redefinitions were, in that respect among others, precursors of contemporary "behavioralism" in the field. The prevalent conception of the scope of politics seemed to them the first and foremost impediment to the achievement of a "science" of politics, in part because of its very narrowness, in part because the phenomena considered political seemed too complex and various to serve as the subject matter of a science. In a sense, the very "uniqueness" used to justify the autonomous study of state-organizations furnished the main ground for their attacks on the conventional delimitation of politics as a discipline.

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[11] E.g., Talcott Parsons and Neil J. Smelser, *Economy and Society* (Glencoe: Free Press, 1956).

[12] This criticism does not apply to works that do not define politics functionally but urge a functional approach to the subject otherwise defined. For example, Almond's widely influential functional approach to the study of political systems is mainly tied to a conception of politics that stresses, à la Weber, an instrument of action rather than a social function: legitimate coercion. In his definition of 1966, legitimate coercion (plus territorial jurisdiction) is all that is advanced; in the definition of 1960, functional language is merely added to the original notion—probably to make the definition of the subject look as functional as the approach to its study. (See note 10, above, for references.)

[13] Bourricaud: Political structures have the task of forging a common will out of conflicting wills in a society. Spiro: Political structures are those institutions of a society that "process" their "issues." See François Bourricaud, "Science politique et sociologie," *Revue française de science politique,* 8 (June 1958), and Herbert J. Spiro, "Comparative Politics: A Comprehensive Approach," *American Political Science Review,* 25 (September 1962), 577–596.

[14] Parsons, *Structure and Process in Modern Societies* (Glencoe: Free Press, 1960), p. 42.

A typical argument to this effect is Catlin's.[15] Science, Catlin argues, induces generalizations from "simple" phenomena that are "commonplace and frequent of occurrence"; states have been and are few in number, and are also quite "uniquely" constituted (i.e., complex and variable on many dimensions); hence nothing can safely be induced from their study. The problem is readily translatable into statistical language: it is difficult or impossible to generalize if one has a small n and a large variance. The solution must be to find a far simpler subject matter embodying many more instances. That subject matter must certainly be of the essence in state-organizations, but, just as certainly, it must also be found in many other contexts.

Note that the problem discerned was not that of finding "equivalents" of state-organizations in premodern cultures. The problem was to find something to replace the focus on the state in *all* cultures.[16]

Despite differences in language, the solutions proposed, like those of the functionalists, converged on a common point. In essence, it was proposed that politics be equated with what might be called "asymmetric" human relationships. All the early revisionists started with the same question: what are (in Catlin's chemical metaphor) the "elements" from which all political complexes, as thought of conventionally, are compounded—or, in different metaphors, the particles or cells of "bodies" politic? The answer was derived from what seemed (and no doubt is) an obvious truth: governments are *complexes* of rule and hierarchy (of κράτος and –αρχος). Hence the elementary relations constituting them must be *items* of power, or control, by some men over others. In essence, these items consist of interactions in which abilities to produce intended effects and derive benefits are unequally distributed. This is precisely what is meant by "asymmetric" relations; as Lasswell might put it in his pungent way, they are relations in which someone affects more than he is affected, controls more than he is controlled, and/or gets more of what is allocated. Such asymmetries manifestly occur in virtually all human relationships, everywhere and on all social levels. They certainly are intrinsic to state-organizations, but states are just particularly large, complex, well-patterned, persistent, and (more questionably) "pure"[17] complexes of them. Asymmetric relations thus provide just the sort of simple, frequent, and commonplace units of analysis that inductive science seems to presuppose.

Anyway, so the argument went—and it surely is not, *prima facie,* unpersuasive. It would be hard to argue that asymmetry is not inherent in governments of all types, at all social levels, or that anything of great consequence about the governments of states would be missed by considering them large and complex networks of power and control relations. Easton does point out that "goals"—the purposes to which power and control are put—also matter.[18] He is right; but nothing precludes analysis of the substance as well as process of politics, no matter how politics is conceived. After all, nothing prevented those concerned with state-organization from analyzing their policies. Nor does the conception of politics as asymmetric relations (*pace* moral critics like Franz Neumann) necessarily have misanthropic implications. It does not imply, as Neumann argued it did, that all human collectivities are ultimately controlled by brute coercion, or that there are no absolute moral laws, or that democracy and constitutional government are impossible, or that no important qualitative differences of any consequence exist among polities, or even that asymmetry is always strongly weighted in favor of nominal rulers.

One is tempted to conclude, therefore, that when political scientists embarked on the search for a less restrictive conception of politics in the 'fifties, they leaped into functional definitions through insufficient consideration of available alternatives or because of moral biases without foundation. Nevertheless, one may have important reservations about the early structural redefinition. It seems better regarded as a large first step toward a serviceable conception of the field's subject than as a definitive delimitation of its scope.

Most important, the equation of politics with asymmetric relations delimits very little. Like the functional conception of politics as the order-maintaining aspects of society, it broadens the field by virtually eliminating its boundaries altogether. To be sure, there are social relationships that are symmetric, or as nearly so as makes no difference. One thinks of buyer-seller exchange relations (under free-market conditions), affective relations among friends or spouses, or interactions on collegial decision-making bodies that are not just nominally collegial. But while such relations frequently occur, they "characterize" few social units; they do so less and less as the notion of symmetry is more strictly defined; they are virtually nonexistent in many societies and subsidiary units; and they are more likely to occur in ephemeral interactions than in organized or institutional collectivities. Certainly, it would be difficult to say that any concrete institution or relationship in any society is automatically excluded from the scope of politics defined as the universe of asymmetric relations.[19]

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[15] George E. G. Catlin, *A Study of the Principles of Politics* (London: Allen and Unwin, 1930).

[16] Catlin was by no means the only man to perceive the problem, or to try to solve it. His contributions were in fact overshadowed by those along similar lines of Harold Lasswell.

[17] "Pure" means "unmixed with symmetric interactions" here. For the reasons that this is questionable, see below.

[18] Easton, *The Political System,* p. 117.

From this difficulty others follow. One involves our subjective interests. Easton (see Appendix) rejects the equation of politics with asymmetric relations mainly because of this consideration: political scientists are simply interested in "political" asymmetries, not all social asymmetries. A general theory of power, he argues, would help them, but would still have to be shaped to the special phenomena of political power, conceived in some narrower sense.[20] More important is the question whether conceiving politics more narrowly (but not in the very narrow sense of state-organization) is advisable also on "disciplinary" grounds—that is to say, whether a general theory of asymmetric relations, even if helpful, would help much. A strong argument to the effect that it would not can certainly be made.

The crux of this argument is that the world of asymmetric relations varies extremely on many dimensions, so that the classification of human relations into symmetric and asymmetric ones would not be very informative descriptively, hence also not in regard to theory. Asymmetric relations vary, for example, on the basis of numbers of people involved in them (from two to just about all human beings). They vary in regard to continuity and regularity of interaction—asymmetries exist in continuous and regular interactions in a persistent social collectivity (teacher and pupil, in school), or in regular but hardly continuous interaction (doctor and patient), or in ephemeral interaction (criminal and victim, or the slow driver and the drivers he slows down). They vary in regard to the nature of the "actors" involved in asymmetric interactions, who may be single individuals or more or less complex collective individuals. They vary in regard to degree and direction of asymmetries—i.e., they may, in varying degrees, involve unidirectional or multidirectional flows of power and control (in certain, probably most, respects, bosses control their secretaries; in others, secretaries run their bosses). And so on, along many more criteria of discrimination, any of which might be consequential for one theoretical purpose or another.

It is possible to cover all this diversity, and more, with generalizations of some sort, and no doubt some people's interests will run to such an extreme level of generality, or beyond. But the distinction between symmetric and asymmetric relations surely discriminates no more than the first and simplest distinction biologists make among members of that extremely heterogeneous class, the animal "kingdom": protozoa (one-celled animals) and metazoa (many-celled animals). For most purposes, a field so broadly delimited would have to be subdivided into many virtually autonomous subdisciplines, so that, as with the functional alternative, we come back again to our starting-point: trying to find some delimitation of "political" asymmetries that seems defensible on disciplinary grounds and at least approximates special subjective interests.

Delimiting the Scope of "Politics" as a Problem in Classification

Working out a reasonable delimitation of the scope of political phenomena is, of course, a matter of classification: arriving at the defining traits of a special class of cases to which the label "political" is sensibly applied. What we have uncovered to this point is a rather familiar problem in the construction of taxonomies (familiar, at any rate, in fields that take classifications more seriously, and construct them less arbitrarily, than political scientists do). To clarify that problem and point the way to a reasoned solution of it, we now go off on a brief tangent on the subject of constructing classificatory categories.

Taxonomies that do not just amount to arbitrary labeling can be constructed by numerous methods, which could only be treated fully in a separate paper. However, much the best of these methods for most purposes, as well as the most widely used where classification is regarded as an important intellectual enterprise (e.g., in biology), is a procedure that might be labeled "progressive differentiation" (my term). That method happens to be especially useful if one is caught in the dilemma of being either excessively restrictive or excessively extensive in one's conception of a class of cases—precisely the difficulty uncovered here.

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[19] Catlin was quite aware of this problem. (See *A Study of the Principles of Politics*, p. 66.) He specifically counsels that politics, as a discipline, must not concern itself with "all the life of the community," only with those activities in it that are, somehow, political. On the other hand, recognizing that not much is left clearly nonpolitical by his definition, he also counsels Aristotelian wholism in holding that politics is present in all social relationships. Easton rightly extends this view to Lasswell's conception of politics, although not to his substantive work. (See *The Political System*, pp. 119–124.)

[20] Easton, p. 123.

"Progressive differentiation" proceeds, in gist, as follows: (a) One begins with a very broad and heterogeneous subject matter, perhaps having nothing but one attribute in common, such as "organisms": material entities capable of reproducing themselves. (b) One then asks on what basis that subject matter can be subdivided into two (or at any rate a very small number of) very populous subsets, so that (1) all instances of the subject matter fall into one subset or the other, and so that (2) all cases in a subset share one characteristic additional to that of the whole subject matter, which (3) is not found in any case in the other subset(s). (That initial cut, in biology, is made between plants and animals, on the basis of ability to produce "food" by processing inorganic matter.) (c) One then asks the same question of the subsets (e.g., animals, as stated, are divided into protozoa and metazoa) and continues thus to differentiate until one arrives at a compelling stopping point—the most compelling being the point where differentiations can only be made sensibly by singling out particular individuals, not sets of them.

The process involves the systematic use of "branching." Each offshoot, at every level, is a member of a more inclusive branch, but has at least one characteristic different from all other offshoots from the same branch. Figure 1 portrays the process graphically (using biological categories—somewhat simplified—to identify levels of differentiation).

Classificatory schemes constructed by progressive differentiation have several characteristics that such schemes ought to have if used as bases of general propositions. They leave nothing "residual" (unclassifiable), preclude ambivalence (i.e., having equally good reasons for classifying a case under two or more sets),

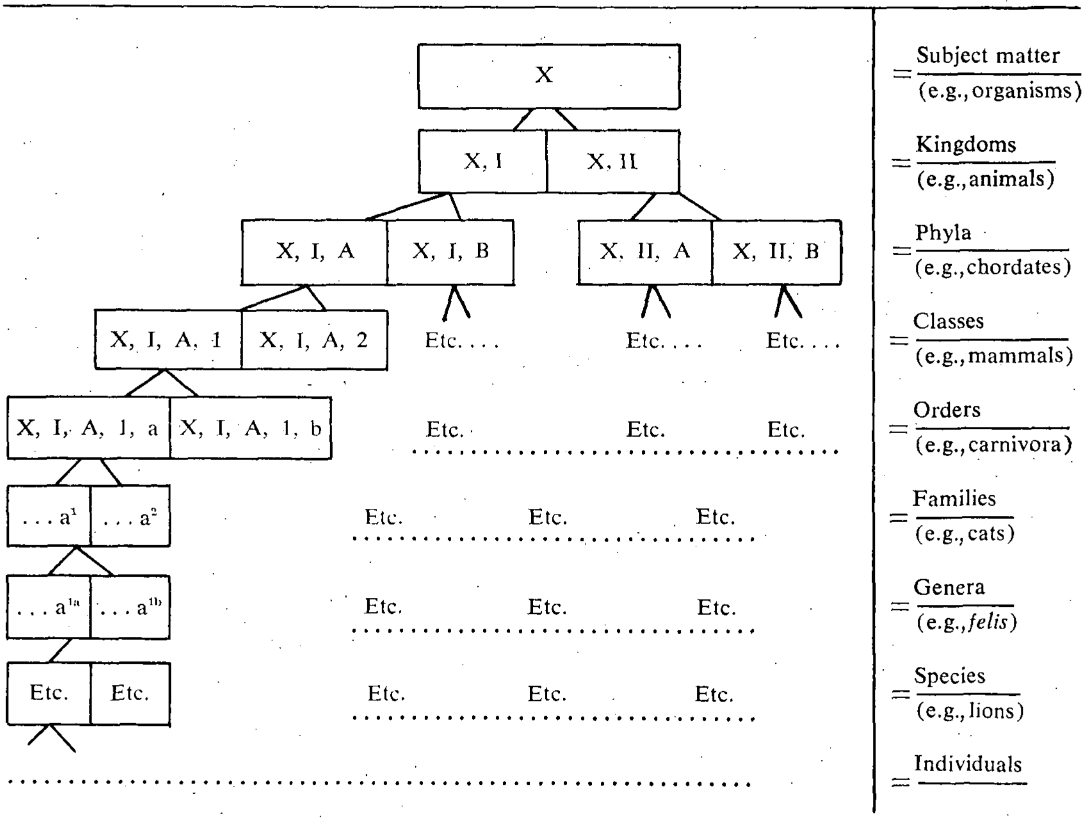

Figure 1. The method of progressive differentiation.

and also preclude ambiguity (difficulty in deciding to what set a case belongs). Even more important here, it should be evident that at the upper levels of a classificatory ladder thus constructed, cases will be very numerous and very heterogeneous, while at the lower levels they will become progressively more homogeneous —similar or identical on many dimensions— and more limited in number. Consequently, while generalizations are possible at any level, those that cover the more inclusive (less differentiated) categories will, of necessity, provide only limited information about the less inclusive classes, and sometimes hardly any of the information one subjectively wants about them. In that sense, they will be "vacuous." On the other hand, a class may be so narrowly defined that subjectively sought information becomes available only at a disproportionate price in regard to the possibilities of induction, or validation of theories arrived at by any means whatever. In such a case, one is compelled to seek a trade-off between the extensiveness and homogeneity of the classes. Even if that compulsion is not absolute, any large gain in extensiveness is to be preferred if the cost in loss of homogeneity is small—not just because of the requisites of inductive methods but because of the parsimony of theories about a subject, in the sense of ratio between cases covered and information provided.

My criticism of others' ways of delimiting the scope of political study can now be restated in a more compact manner. In addition, the tack to take in order to solve the problem we have uncovered seems clearly to emerge from this note on methodical taxonomy.

(a) *The conventional delimitation of the scope of the field sacrifices too much extensiveness for the sake of homogeneity.* It does so not just because state-organizations are rather few in number, but also because they are not so different from other structures as to make the gain in homogeneity large. This is not to mention the nonmethodological fact, certainly admissible as a supplementary argument, that our subjective interests no longer are confined to so exclusive a subject matter, although they certainly continue to include it.

(b) *The early revisionist conception of politics as the study of asymmetric relations sacrifices too much homogeneity for the sake of extensiveness.* It does so chiefly because Catlin and his ilk seem to assume that typological categories should have only a single defining characteristic, "commonplace and frequent of occurrence." Obviously they may have any number of them, so long as the mix between subjective interests and disciplinary imperatives remains reasonable. (In fact, the broad structural conception of politics itself has at least three: (1) homo sapiens, (2) in relationships, that (3) are asymmetric.)

(c) *The later "functional" revision has the same result, compounded by difficulties that always arise when one emphasizes functions rather than structures.* It proceeds, implicitly, from the right question: to what more numerous, if less heterogeneous, class can "states" be considered to belong? But the answers provided lead to the same overextensiveness and excessive heterogeneity as do the earlier revisions and, still worse, direct attention to elusive abstractions from concrete observables.

(d) *Our problem then is to find a reasonable trade-off between the homogeneity of the conventional conception of the scope of the field and the inclusiveness of proposed revisions, without incurring avoidable costs in ability to identify pertinent phenomena.* To avoid vagueness, we want a "structural" conception of political phenomena. For the sake of homogeneity, we do not want to include all asymmetric relations, although we do want political phenomena to be a distinct subset of such relations. For inclusiveness, we do not want the conception to be limited to state-organizations, although we do want such organizations to be a distinct subset of the class.

This clearly calls for adding structural criteria of discrimination to that of "asymmetry." The full set of traits must be characteristic of state-organizations and also of much else, and yet still delimit a rather homogeneous and precisely identifiable set of cases.

Here we can leave criticism, and turn to the constructive part of the argument.

Political Relations as a Subset of Asymmetric Relations

My position is that three criteria additional to asymmetry can be used to define the nature of political phenomena in a manner that reconciles our subjective interests with disciplinary requirements. Stepwise, they delimit successively narrower "populations" suitable to increasingly informative empirical theory—populations that increasingly resemble the phenomena central to the traditional subjective interests of political scientists, but that are not in the least culture-bound, nor so intrinsically limited in number or widely various as to rule out rigorous inductive procedures.[21]

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[21] It is probable that the criteria will seem "obvious" when stated. If so, all the better—and all the more reason for making them explicit. Points obvious when stated, after all, are not always obvious when not stated. The unfortunate tacks others have taken in exercises of this kind attest to that.

(1) *Asymmetric relations may or may not occur in social units, among people acting in their capacities as members of the units. Those that do thus occur may be considered political, others not.*

This criterion subtracts from all power and control relations a large number and variety that are especially far removed from the commonsense conception of politics, often quite amorphous, and likely, if not subtracted or distinguished, to make theories vacuous. Examples are the power of a criminal over his victim or the slow driver over the queues in his wake. Such relations are clearly asymmetric and satisfy formal definitions of control (or power), such as that of Dahl and Lindblom:[22] "B is controlled by A to the extent that B's responses are dependent on A's acts in an immediate and direct functional relationship." But they do not occur in social units, or do so only in a trivial sense (e.g., in that they occur among members of the same nation).

To avoid ambiguity, and the kind of definitional carping it begets, we had better be precise about what a "social unit" is (although readers no doubt have an immediate feel for the term).

(a) Social units are not mere aggregates of persons, but, so to speak, collective individuals. That is to say, they have a certain identity of their own, apart from the identities of their members—a fact generally ratified by special proper names (the United States, New Jersey, Princeton University, the Woodrow Wilson School, etc.). They thus persist despite turnover in members, and often have legal status quite apart from that of the individuals that constitute them. They are, in the medieval sense, "corporations," i.e., bodies or entities in their own right.

(b) In such collective individuals, interactions are recurrent and patterned: i.e., they occur continuously, or at least very frequently, and can be generalized about with rather high predictability. They are not sporadic or isolated encounters—not, for instance, the sort of relationship that exists between buyer and customer but the sort that employers have with their employees.

(c) However much the unit may be identified by its members, the members also identify themselves in terms of the unit; in one degree or another their personal identity is perceived by themselves as that of the larger entity. One does not "identify" oneself by saying: "I am a person held up at gunpoint last January 24"; one does think of oneself as "a professor at Princeton University," and the like. The strengths of such personal identifications with collectivities vary, but some tendency to define the self by the collectivity can always be found in "social units."

(d) As corporate entities, social units have goals distinct from those of individual members *qua* individuals. Indeed, individual satisfaction, like individual identity, is itself defined, in some degree, by collective goal-attainment. In the case of families like those Banfield studied in Southern Italy,[23] that definition is extreme, but it exists in virtually all families. As societies "develop," however, the identification of self-satisfaction with collective goals seems to become focused increasingly on larger-scale units, including the "state" and organizations like political parties. Old equations between self and collectivity, by and large, remain, but others (hence other social "units") are superadded. (I leave aside the vexing question of whether there are "rational" collective goods. Empirically detectable nonrational ones suffice.)

(e) As in other "bodies" there exists in social units a more or less specific, or differentiated, allocation of roles or division of labor attuned to the unit's goals, together with definitions of tasks, duties, responsibilities, and rights appropriate to the roles. This, again, is as manifest in small-scale units like families as in large-scale ones, like states. Someone is charged with the shopping, the cooking, the cleaning-up, the bill-paying, much as roles are devised to legislate, administer, adjudicate, push interests, or aggregate votes. The allocation may vary among members almost daily, and also may be quite uninvidious, but generally it both exists and remains fairly fixed, if only because of variations in capacity to perform tasks or expertise acquired in performing them.

We clearly eliminate many of the problems, subjective and disciplinary, raised by the broad conception of politics as social asymmetry by confining the field to social units thus conceived. Most of all, we avoid that of arriving at uninformative generalizations which cover both asymmetry in social units and in encounters among individuals outside of them—generalizations perhaps as vacuous as that the controller tends to command a greater stock of "pertinent values" (force, position on a highway, etc.) than the controlled.

(2) *Asymmetric relations may, or may not, occur among hierarchically ordered segments or members of a social unit, or manifestly affect relations among such segments. Those that*

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[22] Robert A. Dahl and Charles E. Lindblom, *Politics, Economics, and Welfare* (New York: Harper and Row, 1953).

[23] Edward Banfield, *The Moral Basis of a Backward Society* (Glencoe: Free Press, 1958).

occur among hierarchically ordered segments, or manifestly affect their relations, may be considered political, others not.

Segments of a unit are hierarchically ordered if any of them are thought of as "superior" to others—literally, at a higher level—so that the unit appears to have "stratiformity." (The term is borrowed from geology, to avoid the probably invidious connotations of the more familiar term stratification). In formal organizations, such stratiformity appears on organization charts and, often, through ranking by title: permanent secretary, undersecretary, assistant secretary, etc. In other cases, it is simply a matter of implicit perceptions: e.g., of parents or teachers by children and by themselves.

Stratiformity, in the above sense, is surely a characteristic of governments (as conventionally conceived) and of most other social units. On the other hand, there (probably) are social units in which avoidance of stratiformity is deliberately sought. More important, there are many interactions within a social unit that occur on the same level of its hierarchy and do not manifestly affect relations among the hierarchic levels. Examples are social relations among legislators or bureaucrats, or, in a small-scale unit, certain interactions, unnecessary to specify, between spouses or children in families. Very indirectly, these may, of course, affect relations among the levels (cocktail parties in Washington may promote voting coalitions, or the children's quarrels may turn mild parents into temporary tyrants). But these are usually remote and highly unpredictable effects, like those that encounters outside of social units might cumulatively have on more comprehensive units in which they occur. They certainly do not "manifestly" affect between-level relations in the sociologists's sense of the term, i.e., in the sense of having intended and recognized effects on social relations.[24] They are thus not in the least like interactions among nominal peers, such as members of a board of directors, that are intended to make rules for a social unit and recognized to do so.

The second criterion of discrimination thus narrows the field by subtracting interactions within social units that most resemble encounters outside of them, which seems desirable from the standpoint of disciplinary considerations. It also narrows it in a direction dictated by the focus of our subjective interests. But it hardly narrows it extremely.

(3) *Asymmetric relations within social units, occurring among or manifestly affecting between-level relations, may or may not be concerned with the direction of the units. Those concerned with their direction may be considered political, others not.*

The "direction" of a social unit refers to four matters that are indivisible but distinguishable, and, in any event, have to be discussed seriatim. One involves a cluster of activities, the second a set of role-incumbents, the third a set of instruments by which the role-incumbents perform the activities, the fourth the activation of a set of sentiments toward the activities, instruments and role-incumbents.

(a) Direction as *activity* involves the general management of the unit: defining its goals and means to attain them; specifying proper conduct within it and, in a sense, "policing" members to assure that their conduct conforms to specification; allocating roles within the unit, together with special tasks, duties, or rights associated with the roles; and coordinating roles and conduct to achieve effective performance in regard to goals or, in a more diffuse sense, to minimize conflict and confusion.

(b) Conceivably all members of a social unit might participate equally in performing these activities on a continuing basis. However, such utopian "communal" arrangements rarely, if ever, exist, nor are they even closely approximated. (In any case, they would not be "asymmetrical.") In virtually all cases, one finds special *role-incumbents* who have assigned or assumed rights or responsibilities to see to the unit's "management," in the various senses of the term. Such role-incumbents may be called the unit's "directors." Their activities in the unit are partly analogous to those mechanisms of control in organic "systems" that keep the organism in equilibrium under stress or prevent stresses from arising in the first place. They are also partly analogous to the activities of engineers who define the objectives of mechanical systems and generate or manipulate structural capabilities and other resources to attain them.

Incumbents of such special roles are always, by virtue of their special rights or responsibilities, superiors of others. However, the term "superordinates" seems more apt a label for them. The obvious reason is that a superior may simply be thought of as higher in status than other unit-members without in any sense being a director in the unit—general status levels and levels of direction need not coincide, and ought not to be made to do so by definition. A superior becomes a superordinate when, in addition to nominal superiority, he exercises a considerable amount of actual control over the unit. Here too, however, a modification, especially stressed by Dahl and Lindblom,[25] is in

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[24] Marion J. Levy, Jr., *The Structure of Society* (Princeton: Princeton University Press, 1952).

[25] *Politics, Economics, and Welfare*, p. 94.

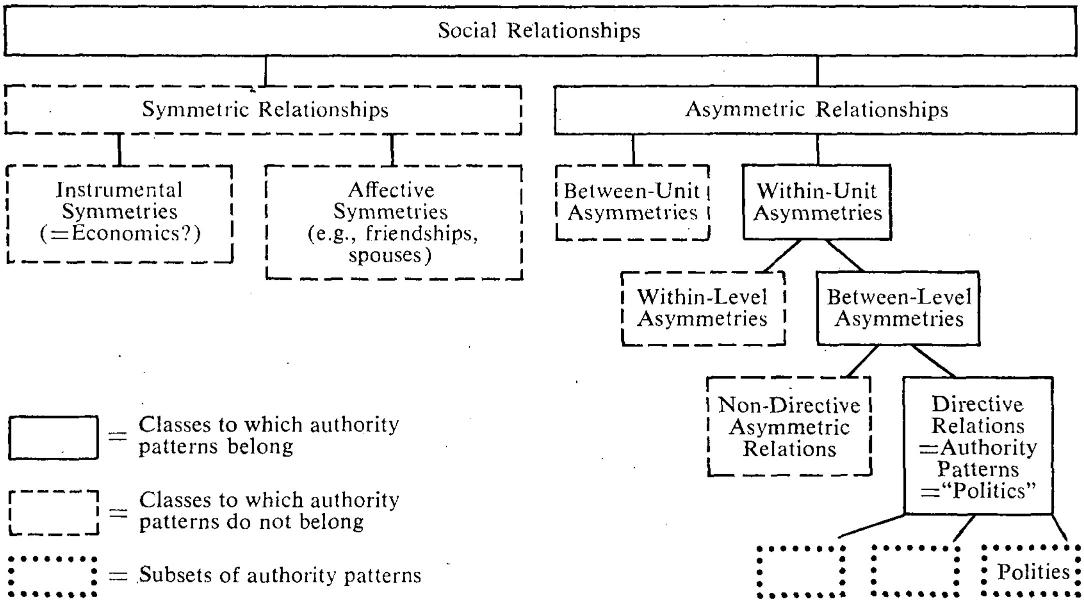
Social Relationships
Symmetric Relationships
Asymmetric Relationships
Instrumental Symmetries (=Economics?)
Affective Symmetries (e.g., friendships, spouses)
Between-Unit Asymmetries
Within-Unit Asymmetries
Within-Level Asymmetries
Between-Level Asymmetries
Non-Directive Asymmetric Relations
Directive Relations =Authority Patterns ="Politics"
Polities
= Classes to which authority patterns belong
= Classes to which authority patterns do not belong
= Subsets of authority patterns

Figure 2. The Place of Authority Patterns in a Taxonomy of Social Relationships.

order. Flows of control between directors and those who are directed need not be, and in fact rarely are, entirely unidirectional. By any formal definition of control, the actions of subordinates can control those of superordinates no less than vice versa, either in latent effect or because manifestly meant to do so—i.e., meant to influence the superordinates' actions. Subordinate actions concerned with directives other than those that merely carry out what superordinates ordain are thus also part of a unit's structure of direction. Such actions may, of course, matter more or less. But even where they are of great import, superordinates have a special role in the determination of the unit's network of direction, either in the sense of quite autonomous power inherent in their roles, or in the sense of aggregating, selecting, or otherwise processing inputs from subordinates, or both.

(c) Direction is carried out by means of *directives:* prescriptions potentially backed by sanctions, negative or positive, if disregarded by unit-members. Directives may take the form of petty commands, broad rules, or implanting in members internalized norms reinforced by the use of deprivations when not acted out. The term "directive" is sometimes used by sociologists and psychologists to denote any guide to action that compels particular choices among alternatives or limits the range of perceived alternatives, and thus reduces the possibility of "anomie" as a psychic condition.[26] Here the sense of the term is narrower: not any guide to action, including all habits and sentiments of morality and propriety, but explicit guides to action within social units, or sometimes interactions with other units in one's capacity as member of a unit.

(d) However, an important aspect of direction, the roles of directors, and the perception of directives is precisely that they do evoke special sentiments of morality or propriety; more precisely, they activate perceptions of *legitimacy*. Such perceptions can be either diffuse or specific. In the diffuse sense, they involve general perceptions of the rightfulness, or justice, of the overall constitution of a pattern of direction, hence its worthiness of being supported. In the specific sense, they involve sentiments of obligation to comply with directives because invested with a special moral quality derived from that diffuse sense of rightful constitution, and therefore of the propriety of punitive sanctions in cases of noncompliance which, otherwise employed, would be perceived as immoral. The perceptions need not be positive. All that is stated here is that, positive or negative, they are evoked. It is inconceivable that such perceptions might not be evoked where obligations are prescribed, where the ability to prescribe them is differentially allocated, and where the behavior used to police and enforce prescriptions is not regarded as morally rightful in any relationship, under all circumstances.

The third criterion of discrimination thus focuses attention on a special cluster of between-level relations within social units. It subtracts relations that are not concerned with their direction in the above senses, which are probably the great majority. The reasons for the subtraction again are both disciplinary and subjective. From the disciplinary standpoint, the important points are that "rule" in any form must have to do with rules or their obvious equivalents, and interactions that are not concerned with rules are both extremely heterogeneous (even within units, between levels of stratiformity) and lack attributes of those that are. The most manifestly lacking attribute is legitimacy: they do not activate perceptions of it, but only arouse general sentiments of morality which, in one degree or another, are modified when the quality of legitimacy is discerned. As for subjective interests, the essential point is that "direction," in all the senses given that term here—the management of a social unit, the existence of superordinate roles, the prescription and policing of directives, and the perception of special moral qualities that justify rights, privileges, and punitive behavior otherwise deemed immoral or amoral—is intrinsic to commonsense notions of government.

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[26] Sebastian De Grazia, *The Political Community* (Chicago: University of Chicago Press, 1948).

The Nature of Authority Patterns: A Summary

The special class of asymmetric relations delimited by these criteria of discrimination may be called *authority patterns.*

The accompanying figure portrays the way the class was worked out as an exercise in progressive differentiation.[27] As should be evident, it implies that some generalizations about social relationships as such should also pertain to "politics," as should some generalizations about all asymmetric relationships, as well as within-unit and between-level asymmetries. Thus, as we pass down the branch-points, an increasing number of generalizations about more inclusive subjects become pertinent to politics; however, the theoretical understanding of politics should also require a good many generalizations about the less inclusive class alone.

The case for delimiting the scope of politics at the indicated level of discrimination still has to be made; so far only groundwork for it has been provided. Before we proceed to that most critical part of the argument, however, two brief sets of remarks are still required. To prevent confusion, my notion of the nature of authority patterns should be contrasted with other uses of the term "authority" and, to make an extensive argument succinct, it should be more directly and summarily stated.

(1) The concept of "authority," like others of its ilk (power, control, influence) has had a checkered definitional history in social studies.[28] The term is probably used most widely as Weber meant it.[29] Weber, to my knowledge, never offered a detailed formal definition of authority, and his use of the concept is ambivalent in certain senses.[30] He does, however, consistently use it in one way different from the way it is used here. He uses authority as a special quality that an exercise of "imperative control" (in my terminology, direction) might or might not have: *positive* legitimacy. I use it as a label for a structure in which activities, roles, and instruments evoke legitimacy sentiments, *positive or negative.* This avoids some knotty problems eminently worth avoiding: how to measure levels of positive legitimacy; at what level of positive legitimacy "power" (or control, or what have you) becomes authority; whether directors can be "authorities" to some people but not others in the same unit, in regard to some directives but not others, at one point in time but not another; whether the absence of negative legitimacy perceptions equals the presence of positive ones—and many others with which no one has yet managed to come to grips. It is surely enough to know that perceptions of legitimacy are evoked to know also that one is in a special universe of asymmetric relations different from others (for which terms like power or control are quite serviceable).

(2) The proposed conception of an authority pattern can be summarized in a general formal definition, elaborated in more specific definitions that are needed to make sense of the general conception:

General definition:

An *authority pattern* is a set of asymmetric relations among hierarchically ordered members of a social unit that involves the direction of the unit.

Subsidiary and additional definitions:

1. *Social units* are collectivities of individuals that may be regarded as collective individuals (as discussed above).

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[27] I am aware that, as a taxonomy of social relationships, the figure (and the argument it represents) is incomplete. The reason is that I am not, at this point, concerned with all social relationships, only with the place of political ones among them. No doubt, however, this involves a weakness: a full, methodical taxonomy of social relationships conceivably might, but probably will not, look different.

[28] For a sense of the confusion surrounding the term, see Robert L. Peabody, *Organizational Authority* (New York: Atherton, 1964), chaps. 1–2, and Carl J. Friedrich, ed., *Authority* (Cambridge, Mass.: Harvard University Press, 1958).

[29] Max Weber, *The Theory of Social and Economic Organization* (New York: Oxford University Press, 1947), p. 153.

[30] Note, for example, the following ambivalence in Weber's use of the concept: (1) Authority is held to exist in *many* types of "corporate groups" (i.e., in our terms, social units) and to involve "the legitimate exercise of imperative control." An aspect of such control is the use of coercion regarded as rightfully exercised. (2) The *state* is defined by its "monopoly of legitimate coercion." Clearly (2) contradicts (1) unless (1) is confined to "stateless" societies. The ambivalence arises, in my view, from the wish—more typical of political scientists than of sociologists—to make too rigid distinctions between polities and other social units.

2. *Hierarchic order* exists where members of a unit are perceived as ranked in levels of superiority and inferiority.
3. *The direction* of a social unit involves the definition of its goals, the regulation of conduct of its members, and the allocation and coordination of roles within it. Direction is carried out through directives prescribed by incumbents of superordinate roles in the unit.
   3.1 *Directives* are prescriptions of conduct that evoke legitimacy sentiments. Such sentiments raise the question of whether prescriptions of conduct ought to be regarded as obligatory and rightfully backed by sanctions.
   3.2 *Superordinates* are any unit superiors who are assigned, or assume, special responsibility for arriving at directives and seeing to it that they are carried out. (In Dahl and Lindblom's terminology, they are both superiors in the unit and, to a substantial extent at least, its actual "controllers.")
   3.3 *Subordinates* are, residually, any other unit-members. It should be noted, however, that in cases of complex stratiformity a member may be both superordinated and subordinated vis-à-vis different other members. Sometimes, in cases of complex differentiation along functional lines, a member may be both a "super" and a "sub" vis-à-vis the same other members, in regard to different activities—an ambiguity that disappears if role-incumbents rather than concrete individuals in all their aspects are treated as a unit's "members."
4. *Super-sub relations* (a self-explanatory term) are at the core of any authority pattern: there is no direction of a unit without a flow of directives and actions enforcing them from supers to subs, and cases in which subs do not try to influence supers in some degree and manner are probably very rare. But since supers generally interact in arriving at directives, either as peers or in a special stratiform structure of superordination, *super-super relations,* insofar as they "manifestly" concern a unit's direction, are also an aspect of authority patterns. And since subs may interact in trying to influence the directives of the superordinates, those *sub-sub relations* manifestly concerned with doing so may be considered a further aspect.

The Case for Equating "Politics" with Authority Patterns

My argument is that authority patterns, as delimited and defined in the two preceding sections, are a sensible point at which a decent trade-off between inclusiveness and homogeneity can be struck, and subjective interests reconciled with disciplinary considerations. That argument rests on three main bases. Each of these is addressed to particular difficulties and advantages of conceptions of the field's scope that others have proposed—plus some supplementary considerations that emphasize the signal importance of studying authority patterns, whether or not taken as defining the subject matter of the field.

(1) *The conception of political study as the study of authority patterns is sufficiently broad (deals with phenomena sufficiently commonplace and frequent of occurrence) to serve as a proper basis for inductive science.* That is to say, the conception serves the purpose of those who first attacked the narrow traditional delimitation of the field's scope, without incurring the disadvantages of the alternatives they proposed.

"Societies"—a term that refers simply to the most inclusive "collective individuals"—consist of very large numbers of such patterns in every case, some very simple and small-scale (e.g., nuclear families), others much more complex and extensive. True, authority patterns at their simplest are not so simple as "acts of control," being clusters of such acts. But nothing prevents "analyzing" them into simpler component units, in any of the ways by which any entity can be analyzed into components clustered in a certain manner: by distinguishing "dimensions" on which they vary, or "elements" of which they consist, or smaller-scale "particles" to which they are reducible. It clearly is unnecessary for inductive science to deal always with the most simple and irreducible phenomena that cluster in entities. In fact, virtually all the very "simple" phenomena of science (cells, chemical elements, molecules, atoms) turn out to be clusters of still simpler phenomena, and failure to arrive at the end of the chain of simplicity—if indeed there is such an end—has not handicapped other fields of inquiry. The main requirement surely is that numbers be sufficient to offset problems arising from range of variability (as argued above), and it is hard to see, *a priori,* why in the study of authority any serious difficulties should arise on that score.

(2) *Defining the scope of the field as the study of authority patterns does not make it so broad as to take us far beyond the range of subjective interests, nor confront us with phenomena so heterogeneous as to make informative theory unlikely or to require much in the way of special disciplinary tools for special types of authority patterns.* That is to say, the definition retains the principal advantages of the traditional narrow conception of the field's scope, without incurring its manifest liabilities.

The methodological reasons for including far more within the scope of the field than state-organizations (and very near equivalents) are important, but by no means definitive. If such organizations were in fact as unique as some consider them, it would be self-defeating to subsume them under a more numerous set of cases: what is gained methodologically would perforce be lost in knowledge of the subject matter. But state-organizations, and near-equivalents, are not all that unique. They do differ from other authority patterns but only on a very limited number of variables, the import of which for methods and products of inquiry is unlikely to be large.

For one thing, the idea of sovereignty is just that: an idea—and an idea more normative than empirical. The idea characterized well the pretensions of sovereigns as expropriators of authority, and sometimes justified their successes; it also served the longings for peace and harmony of those caught in struggles between competitive, overlapping authorities. But even if indivisible in principle (we leave that to political philosophers), it has never really been undivided in practice—certainly not if taken to involve the autonomous power to make laws perceived as binding and to enforce them with coercion perceived as morally permissible. Being on the official statute books or civil and criminal codes is not all that makes directives appear obligatory. Virtually every social unit has its own set of directives, mostly unchecked by more inclusive authority, and enforces them with more or less severe punitive sanctions, often of a kind considered impermissible among individuals outside of the confines of the unit. And even if there may be appeal to higher authority, that does not much change the nature of the activity. Normative theorists, like the "pluralists," were the first to perceive this, wishing to deny any unique normative position to the State; empirical theorists ought to follow suit, to deny it any claim to autonomy as subject matter for a discipline.

When comparing state-organizations and other authority patterns subsumed under them by the idea of sovereignty, one finds that the dimensions on which they vary largely coincide. An "analytic scheme" for the less inclusive patterns serves well as an analytic scheme for the more inclusive; a typology of them is serviceable as a typology of "states" as well. We shall really only see this clearly in a set of subsequent papers (now in preparation) devoted to the analysis of authority patterns into constituent dimensions and an attempt to synthesize the analytic elements into clusters serviceable as typology. But we already know from the way the conception of authority patterns was worked out that state-organizations belong to the same "family" (or class, or genus) as other authority patterns at a rather high level of differentiation. It is hard to think of any (nonfanciful) criterion of discrimination that would differentiate further, aside from extensiveness—territory and number of members—and the probably related variable of complexity. Whether these factors make much difference in regard to disciplinary considerations (concepts, methods, problems, theories) is at least an open question that ought not to be settled by definition, especially by a definition that incurs manifest handicaps for inductive science. Besides, states and near-equivalents vary considerably in extensiveness and complexity, and some "private" governments are certainly larger in size and more complex than many public ones. Size and complexity thus seem more useful as potential explanatory variables (as in a forthcoming study by Dahl and Tufte) than as defining characteristics of a field.

If this general argument is correct, then both parts of the general argument above follow: we do not go far beyond the focus of subjective interests in dealing generally with authority patterns, nor define a subject that is vastly heterogeneous, or even significantly more so than the original basis of the field—taking heterogeneity in the literal sense: not "different" (any concrete individual differs from all others) but "belonging to different genera."

(3) *The conception of political phenomena as authority patterns helps to identify the equivalents of state-organizations where such organizations do not exist or are not highly developed.* That is to say, the conception serves the purpose of the functional alternatives to the traditional structural conception of the scope of the field, while avoiding the liabilities of purely functional conceptions.

In a sense, all authority patterns are "equivalents" of state-organizations, i.e., belong to a class to which the latter also belong and which, although numerous, discriminates a great deal, since it does not include most human relationships, symmetric or asymmetric. Authority patterns are equivalents of state-organizations even in the various functional senses that have been proposed: they maintain order, integrate, define, and try to attain goals. Moreover, if scale as such is the critical variable that distinguishes "states" from social units included in them, no difficulty arises in answering the question that the functionalists take as their point of departure: "What is the nearest equivalent of state-organization in societies where it does not exist—in, say, Nuer society, or among the Mayas?" No need at all to resort to "functional" equivalents, an obvious structural one being available: the nearest equivalent is the authority pattern of the most inclusive (or extensive) social unit one can identify in any case in question. We can, in fact, take this as a simple general definition of all "polities" (or governments) as subsets of the class "authority patterns," and thus avoid dubious elements of their definition, such as that they are sovereign wielders of authority, or monopolize legitimate coercion, or alone perform order-maintaining functions in societies.[31]

There is nothing wrong with making a specialty of the study of polities in that sense, or of other subsets of the more inclusive class. Definitions of scope are intended for disciplines as a whole, not for any and all who work in them. What is regrettable is not specialization within a field but equating the whole of the field with the specialty, so that avoidable liabilities are incurred and closely similar subjects neglected.

(4) *Neglecting the study of nongovernmental authority patterns has the consequence not only of leaving unstudied much that is interesting and important in itself, but also of leaving the study of "polities" (however defined) uninformed by much that can be extremely useful for understanding them.* That is to say: even if authority patterns were not taken to define and delimit politics as a discipline, there would be weighty reasons for political scientists to extend their scope of attention to such patterns in all forms. Some of these reasons, however, would not be as weighty as they seem if governments as a special category were vastly more different from other authority patterns than I consider them.

It has often been argued that "private governments" should be far more widely and intensively studied than they are. The reason generally given is not that they are sufficiently like "public governments" to form a subject matter for a single discipline. It is simply pointed out that they exist; that they are interlaced with public authority; and that they are highly consequential—not least for the network of governmental directives that constrain individual lives, provide privileges and rights, and impose obligations, duties, deprivations. The argument, unfortunately, has not been widely acted upon in practice. There are exceptions in political science, but they are out of the ordinary.[32] There are more in other social sciences, particularly in family and industrial sociology, but they too are hardly commensurate with the extensiveness and import of the subject.[33] The consequence, needless to say, is conceptual, methodological, and theoretical retardation. As to reasons for the neglect, there seems to be a growing tendency to consider it part and parcel of a general conspiracy to conceal the networks of power and privilege that dominate supposedly "liberal" societies. Maybe so; but no demons are needed to explain the void. Just as likely is another possibility, i.e., that areas of disciplinary overlap may engender voids as well as duplications. On this hypothesis, political scientists neglect private governments because they are concerned with public ones and do not like to wander far outside their boundaries; other social scientists neglect them because their interests are broader, because they have no specialized competence to analyze them, or because they consider them to fall within the political scientist's domain. One can expect that the current sense of a "crisis of authority" in all institutions in all advanced societies—plus the sense that all sorts of nebulous powers dominate them in uncharted ways—will engender studies that decrease the void. But, on the evidence, studies with normative axes to grind are as likely to retard, or divert, disciplinary advances as to promote them, and they are most fruitful when informed by study conducted from a more neutral basis.

As long as systematic studies of nongovernmental authority patterns are few and crude, we are deprived also of knowledge potentially very useful for making sense of governmental patterns—in at least three ways: (a) Despite pitfalls when one extrapolates from one level of analysis to another, there is much to be learned about macrocosms from microcosms.[34] The smaller units, moreover, often accommodate better methods of study, and are more accessible to study than the larger ones. One would suppose that the more the microcosm resembles the macrocosm, the less dangerous the pitfalls and the more directly pertinent the things to be learned by extrapolation. (b) If Easton is right, as he clearly is, in saying that political study (in his sense, discussed briefly in an appendix to this paper) could profit from a general theory of power, then it could manifestly profit even more from a general theory of authority patterns—not, in this case, by extrapolation from one level to another, but by the partial application to less general phenomena of theories about more extensive ones. (c) Even if governmental authority patterns are separated from less extensive ones, the latter are likely to be important explanatory variables for aspects of governmental authority, e.g., on the basis of learning processes, or because they provide links in other senses (mediate between) governmental and strictly nonpolitical variables, such as economic ones, social status or mobility, and so on. I have made an extensive case for using nongovernmental authority patterns as explanatory variables (in theories of stability in democracies or, in a more general sense, performance levels of all kinds of government) in other publications; interested readers should consult these for a full statement of the argument. The general point, however, does not rest only on my arguments. Nongovernmental authority patterns have been used, implicitly, as explanatory variables for governmental ones in hosts of work, and have been so used explicitly at least as far back as Montesquieu, or indeed Confucius.

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[31] The notion of extensiveness in fact figures in some definitions of political systems. The most explicit example is Apter's: Polities are the *most generalized membership units* of societies with defined responsibility for maintaining the societies and enjoying a monopoly of coercive power. Other definitions imply the same more awkwardly: e.g., one of Almond's definitions which includes the notion of an "autonomous society"; or the much earlier one of MacIver which includes the notion of a "territorially demarcated community" among numerous other definitional attributes; or Dahl and Lindblom's notion of government as having "the last word" over other organizations in an "area." See Apter, "A Comparative Method for the Study of Politics"; Almond and Coleman, *The Politics of the Developing Areas;* Robert M. MacIver, *The Modern State* (London: Oxford, 1926), p. 22; Dahl and Lindblom, *Politics, Economics, and Welfare,* p. 42.

[32] See Dahl, "Business and Politics," for documentation in one area of private authority.

[33] I refer here to studies mainly concerned with the systematic study of authority patterns—such as that of Peabody. Authority being ubiquitous, references to authority patterns, or potentially relevant materials, may be found in virtually all studies of social institutions, but chiefly in their interstices, between the lines, in connection with other matters, without special emphasis or systematic treatment.

[34] A useful work to consult on both points is Sidney Verba, *Small Groups and Political Behavior: A Study of Leadership* (Princeton: Princeton University Press, 1961).

Note that this delimitation of "politics" is not wholly unprecedented. One can find at least one (implicit) anticipation of it in the modern history of political thought. It is mentioned at this point partly to avoid misimpressions about claims to originality and partly because, indirectly, it adds to the force of my argument. The anticipation (briefly alluded to earlier) occurs in the works of the so-called "pluralists" (Figgis, Laski, Cole) of an earlier period. The pluralists argued, in gist, that the state's claim to sovereignty did not really give it a special *moral* status relative to other social structures. It is true that they were not concerned with the scope of politics as a field of study. They were normative philosophers. Besides, political science hardly existed as a special "field" when they first wrote. However, their arguments did rest on doubt about the very existence of something properly considered "sovereign," or, better, doubt that the special attributes of structures claiming sovereignty made much, or any, moral difference.

How does this reinforce my argument? Simply in that, if the same moral problems (e.g., of rights, obligations, liberty) are raised in a variety of social units, it is probable that descriptive similarities among them are also great. Put differently: if apparently diverse phenomena pose largely the same problems, moral or otherwise, we may take this as a powerful indicator of their being, despite appearances, members of a common taxonomic class.

Politics, International Relations, and "Political Economy"

Objections to my position are less likely to concern what it adds to the conventional scope of political study than what it seems to subtract. Two apparently subtracted subjects are especially likely to engender demurrals: (1) relations among polities and (2) the nonauthoritative, *in general symmetric*, processes central to "economic" analyses of politics, such as political bargaining, political coalition-formation, and the adjustment of governmental programs and party positions to voters' preferences.[35]

These are certainly among the more common subjective interests in political science departments. Moreover, a reasoned case for including both in the political scientist's field of attention can certainly be made. It seems to me, however, that a stronger case can be made to the effect that their elimination from "politics" here is either more apparent than real or else inconsequential. This needs elaboration.

(1) We all know that the study of *international relations* has long played a major role in political science departments. Reasons that this should be the case, as long as polities are central to our concerns, are not hard to find; above all, it is arguable that linkages between the internal affairs of polities and relations among them are such that neither can be properly understood without the other.[36] That argument is somewhat intricate, but the basic position is obvious enough to need no spelling out.

The first point to note by way of rebuttal is that nothing said here rules out the study of international relations. Anyone interested in that special subject can still pursue it.

Beyond this, aspects of international relations, as conventionally conceived, are not excluded by the proposed delimitation of Politics—particularly two in which "linkages" are ignored at most obvious peril. One is the study of supranational integration. Authority patterns above the national level fall within the field's scope no less than those below it. In addition, the study of authority patterns can certainly accommodate studies of how they are formed. Every field theorizes about statics and dynamics, and the study of political dynamics is most fundamentally concerned with the processes by which political units come into, or pass out of, being. Processes of international integration surely are processes that develop directive, asymmetrical relations, within embryonic social units, out of relations falling into other taxonomic categories. (That applies to the formation of nation-states themselves no less than processes that, some argue, are now taking us "beyond the nation-state.") The second aspect still manifestly included in politics is, of course, foreign policy, especially insofar as such policy has directive consequences for members of polities.

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[35] Much of the recent literature on this subject is summarized in R. L. Curry, Jr., and L. L. Wade, *A Theory of Political Exchange* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), and some of its anticipations are summarized in James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962), pp. 323–340. In a sense, most of the literature on political competition, anyway *in democracies*, is concerned with the subject, even if economic postulates or methods of reasoning are not explicitly used.

[36] See, for example, James N. Rosenau, *Linkage Politics: Essays on the Convergence of National and International Systems* (New York: Free Press, 1969).

Nevertheless, other aspects of international relations clearly cannot be squeezed into the proposed delimitation, without procrustean distortion. They fall outside it because they are symmetric in character—more akin to economic market relations than to authority relations; or because they involve between-unit asymmetries; or, farthest removed from our conception of politics, because they involve only indirect impingements rather than "relationships" in the sense of interactions. These aspects of international relations are also "linked" to within-unit affairs, and vice versa; and it is in regard to them that potential disciplinary losses (not losses in status and the like) become serious.

There is reason to think that the losses would not be large, on either side. The disciplinary boundaries between politics and economics, after all, have not prevented studies that link political and economic factors or political studies using economic postulates and methods. Neither have those between politics and sociology, politics and psychology, politics and anthropology, prevented the development of political sociology, studies of political socialization, or studies of primitive political systems. Nor has the fact that economists study markets and sociologists study corporations had very deleterious consequences. Serious scholars who feel compelled to cross disciplinary boundaries do so, paying the price in time and effort. In the case of studies of polities and their relations, the price will certainly not be as great as in many others: neither presently requires mastery of particularly exotic skills or peculiar talents. Moreover, on the evidence, interdisciplinary programs can be useful substitutes for departmental identities, and have the advantage of allowing one to choose by reasoning rather than brute convention among the disciplines to be related.

Here we come to gains that may offset any losses if departmental divorces occur. It is by no means certain that specialists in international relations are best off as general political scientists. At the student, or even faculty, levels they might be better off devoting more time and other resources to studying international relations as such. They might also learn more that would help them make sense of their subject from other fields (e.g., economics, anthropology) than they presently do from various subdivisions of political science—many of which departmental rules and teaching loads force upon undergraduates, graduate students, and teachers. Given the manifest taxonomic differences between polities and international systems, and the resemblances of the latter to other phenomena, this is, in fact, very likely to be the case. By the same token, as already argued, those who study polities may learn more from studying authority patterns at lesser levels than from the study of international relations. (I say "may" because I do not know enough to say "will." Just for that reason, however, choices should be open, not foreclosed.)

One is struck, in this regard, by the paucity of theories, or even mere theoretical approaches or frameworks, that apply both to polities and to their relations (as distinguished from their linkages), except at very general and uninformative levels. This is precisely what one should expect if taxonomic differences are considerable. And yet, simply because scholarly convention and administrative habit identify international relations and politics, people continue to devote sweat to distracting exercises that might justify the habit. They search for definitions of politics that might cover both international and intranational affairs (not hard to do, but also not very useful). They look for any signs in international activities indicating that they are not absolutely different from intranational activities (also easy, since, of course, they are not). They try to develop perspectives of inquiry, theoretical approaches, and theories that might apply to both sets of phenomena (more difficult to bring off, but certainly also possible at a sufficiently inclusive, and heterogeneous, level). The results are often trivial or embarrassing. It would seem less wasteful, on the face of it simply to work with one's materials, allowing any correspondences to emerge naturally from the work, than to strain after commonalities simply so that habit might appear as reason.

(2) Objections to the possible elimination

from "politics" of *symmetric relations that figure in the making of directives* (as well as in processes of recruitment to superordinate roles) should be taken more seriously. For one thing, such relations are what "politics" is often taken to refer to in general usage—e.g., when distinguished from government or administration. More important, the relations involved do not just play a momentous role in state-organizations; they can occur in any and all authority patterns, and probably do in most.

To disarm objections on this score, it should suffice to say that the study of the direction of social units clearly includes the study of processes by which directives are arrived at, carried out, observed or defied, as well as processes by which those who carry out direction are recruited to their roles. The notion of asymmetry merely provides the first of several criteria of discrimination by which the concrete phenomena of the field are identified. If symmetric relations occur among members of concrete units identified by these criteria, and if these relations are manifestly concerned with the direction of the units (in short, intrinsic to authority), they fall within the boundaries here delimited—just as asymmetric relations among members that are "nonauthoritative" fall outside them.

The elaboration of the general definition of authority patterns (above) points to two aspects of the patterns likely to involve symmetries. One is relations among superordinates (*super-super relations*). Superordinates may work out directives by vote-trading, "side-payments," and other such modes of bargaining to form winning coalitions.[37] Such collaboration is not always corrupt or cynical, or even just utilitarian. It would seem that there are cultures in which particular value is attached to collegial behavior and consensual decisions. In cultures such as these, symmetry among superordinates is a general end rather than merely a means for optimizing particular utilities. (It is manifest also that the more a social unit's tacit or explicit decision-rules require assent by numerous participants, the greater the number and significance of symmetries in decision-processes will be.) Symmetries may, of course, also occur in *sub-sub relations* intended to "influence" direction. This does not refer to equal power to influence superordinates. It refers chiefly to exchanges and compromises among subordinates for the sake of forming cohesive influential groups.

Despite all this, it must be granted that the inclusion of symmetric relations in "political" study introduces an element of dissonance, or impurity, into the proposed conception of politics. Symmetries remain symmetries, no matter how justified their inclusion in studies of asymmetric relations and no matter how often one says that they must manifestly affect asymmetries of a particular kind. The conceptual dissonance can be tolerated. But it should be realized that relations in authority patterns which are not themselves authoritative will at least require theories different from those that are.[38]

It is altogether likely that the theories required will substantially resemble, or coincide with, those of economics—the field preeminently concerned with "instrumental symmetries" in social relations. (They may also resemble certain theories of international relations, if and when properly formulated.)[39] The adaptations of economic theories to political relations are, in fact, already well-developed in a new "political economy" that better merits the label than the old (see note 35). These theories link two fundamentally different, but intertwined, aspects of organized or institutionalized human relations: those in which humans are equals and exchange and those in which they are hierarchically ordered and direct or are directed.

Proponents of economic theories of politics often justify their work on grounds that beg questions one need not beg. They claim that "human nature" is inherently Hobbesian or in some more benign sense value-maximizing, and hence that politics and economics must be governed by the same laws. Or they rest their case on the claim that methodological imperatives (parsimony, the deductive derivation of propositions, predictive or explanatory power) somehow compel working with economists' methods on economists' postulates. All this is probably untrue, and surely unnecessary. The political economists need only claim that concrete political structures and processes contain relations of the sort economic theories are designed to handle, so that such theories are a useful basis (prefabricated, as it were) for partial accounts of politics. How partial or comprehensive these accounts can be is best left to be settled, *a posteriori*, by competition between contending approaches.

This argument has several further implications of consequence.

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[37] For a list of such possible "exchanges," see William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale, 1962), pp. 108–114.

[38] This, in turn, should not really bother anyone. No one has ever held that theories of politics can avoid all nonpolitical factors.

[39] If theories resemble both those of economics and international relations, these two bodies of theory should resemble one another. In this connection, note that "entrepreneurial," i.e., economic, theories of international relations are now beginning to emerge. (A good

It manifestly implies that the contemporary political economists' own fixation with state-organization and processes intimately associated with the state (e.g., party competition) is ill advised. Their theories pertain to processes that may occur in any and all authority patterns. Many of these, moreover, are far more suitable to, and accessible for, testing their models—an activity for which they, like other political scientists, are not notable—than are political structures as conventionally conceived.

A second implication is that we should start to develop theories of asymmetric relations—particularly those that occur within social units, between levels, and involve "direction"—as rigorous and fertile as those of instrumental symmetry. Such a counterpart to economic theory certainly does not presently exist even in embryonic form, and the failure of political scientists to define their subject matter surely explains, in large part, why this is the case. In turn, the lack of a powerful "pure" theory of politics helps explain why contemporary political economy is so much more economic than political.

A third implication is related to this point. My argument implies that one should think of "political economy" in two ways, and work toward much greater symmetry of emphasis between them. There is, on one hand, the economics of politics: the study of symmetries in authority patterns. There is, on the other hand, the politics of economics: the study of authority in symmetric relations. I am not referring here to Dahl's still largely unheeded call for the political analysis of business firms as "political orders."[40] The subject is much larger and more subtle. It entails study of the ways authority impinges upon symmetry in any and all social units and relations—not excluding economic markets themselves. Except for economists' theories of monopoly and oligopoly, that subject is much less explored than are economic theories of politics. Assuming that the necessary theoretical tools are developed, a political Downs roaming through the economists' domain might, just conceivably, bring off some eyebrow-raising results.

Appendix:
A Note on Easton's Conception of the Political System

The point of this paper, that authority patterns should define and delimit political study, is (as stated in the text) not claimed to be wholly original—only persuasive. As well as being anticipated by normative philosophers, and following up arguments for studies of "private" governments as important influences on public ones or as not greatly dissimilar from the latter in nature or impact on individuals, it is also in line with points made by many contemporary critics of liberal democracies—especially their argument that liberal governmental institutions conceal oppressive networks of power that act through them or directly constrain individuals. In effect, it simply spells out explicitly what is largely implicit in all these views and rests the case for equating political inquiry with the study of all authority patterns less on normative grounds than on "disciplinary" considerations.

Even in this regard, the argument might appear already to have been developed by David Easton in *The Political System*. Some comments on points of difference between Easton's position and mine are therefore in order.

Easton defines "political acts" as those that "*authoritatively* allocate values in a society."[41] A social structure, therefore, is political if it engages in the authoritative allocation of values. The operative term "authoritative" corresponds to mine, and Easton's position, like mine, was worked out by criticism of the very narrow and very broad structural conceptions of the subject matter of the field, to some extent on similar grounds. (The "functional" alternative was not considered by Easton, since it had not yet become current when he wrote—although he also ignores it in his later work.)

If my argument only restated, in a somewhat different way, Easton's position, it could still be justified on the ground that Easton's influence on the conception of the field has hardly been great—except for the mouthing of definitions—and can stand repetition and reinforcement. Actually, however, resemblances between Easton's position and mine are illusory. Easton's conception of politics (as distinguished from his systems approach to political study) has had no discernible impact on what political scientists do, for the good and simple reason that it really only presents a new verbal way to characterize the conventional conception of the field.

That Easton's position remains closely tied to the notion of political structures as state-organizations, or very near equivalents, is evident in this crucial passage:

> . . . in spite of the fact that for the members of [organizations that are not society-wide their] policies carry the weight of authority, it is at once ap-

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example is Norman Frohlich and Joe A. Oppenheimer, "Entrepreneurial Politics and Foreign Policy," *World Politics*, 24 (Supplement, 1972), 151–178.

[40] Dahl, "Business and Politics," pp. 3–9.

[41] Easton, *The Political System*, p. 134 (my italics).

parent that political science does not undertake to study these policies for their own sake. Political science is concerned rather with the relation of the authoritative policies, made in such groups as associations, to other kinds of policies, those that are considered authoritative for the whole society. In other words, political research seeks first and foremost to understand the way in which values are authoritatively allocated . . . for the whole society.[42]

Processes of direction, directive roles, and internal directives in less comprehensive groups thus remain beyond the disciplinary pale. They are considered only as substantive inputs into "polities" (as defined above)—as parts of the "super-sub" relationships of the most inclusive authority patterns, or matters of boundary-exchange between polities and nonpolitical structures.

Why does Easton delimit the field in a manner so closely tied to a conception of it that he attacks? He does it, in the final analysis, because he only takes into account our conventional subjective interests, not any disciplinary considerations—except when he attacks alternatives. At all points where his restrictive conception is stated he invokes as support for it only that political scientists are "not concerned" with a more broadly defined subject matter. This is an explanation of what they do, but hardly a reason for doing nothing else. Besides, it is manifestly untrue, because some of them actually are concerned with more, and it says nothing about what their concerns ought to be. Values are allocated by the "policies" of all groups;[43] indeed one can go further and hold that values are allocated in some sense not just by "policies" but in all social relations (asymmetrically or otherwise). Therefore, that part of Easton's definition of the subject of the field which refers to value-allocation is clearly gratuitous. Since all policies of all groups may be "authoritative," that part of the definition of politics also delimits nothing specifically political—a point that should obviously have given Easton pause about making "society-wide" relations the only attribute that discriminates between political and nonpolitical phenomena.

It is not widely realized that the operative part of Easton's delimitation of politics is society-wide relations, nothing else. More attention has been paid to the redundant parts—which *appear* to say more than the crucial part of the definition. But Easton's own subsequent work confirms my argument: nowhere has he concerned himself with anything other than governments in the conventional sense. My position can thus be taken as one to which much of Easton's preliminary argument about the nature of political phenomena leads but that Easton himself never reaches. In that sense, it does not reiterate his views but extends them over a rather similar route toward a different destination.

(Readers might note, in this connection, a curious point about Easton's use of sources. In two places, he cites Sir George Cornewall Lewis's *A Treatise on the Methods of Observation and Reasoning in Politics*, published in 1852—a book that precociously anticipates many current methodological issues in political science. He does not cite Lewis's *The Influence of Authority in Matters of Opinion*, published three years earlier, which attempts to demonstrate that authority pervades all of social life, all groups (as DeGrazia puts it)[44] having their own "small common goods" and "small authorities" that "envelop us at birth and clothe us throughout life." Easton may simply have missed that work. It is not hard to miss it, for its influence has been virtually nil. The fact remains that it comes closer to what I argue than does Easton's argument itself.)[45]

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[42] Easton, p. 134.

[43] Easton, p. 130.

[44] De Grazia, *The Political Community*, p. 330.

[45] Ten years ago I wrote an essay on the development of "comparative politics" from its remote ancestry in Aristotle to the present. (See "A Perspective on Comparative Politics," in *Comparative Politics: A Reader*, ed. Eckstein and Apter [New York: Free Press, 1963], pp. 3–32.) The essay concluded with a problem to which, at the time, no solution had been worked out. As long as political studies emphasized formal-legal aspects of state-organizations they had a certain focus and simplicity. The tendency in the field, for good reasons, was to deemphasize that subject matter. But the result of doing so was loss of focus and simplicity and, as a result, a loss of bearings in regard to what the field was all about, especially as a field of *specialization*. Political scientists often did not practice what they preached: they continued, in their work, to deal chiefly with state-organizations, while disavowing the reasons for doing so (in the manner of Easton). Or, if they did follow their own prescriptions, they seemed to equate politics with "the whole of social life in all its facets." The problem, then, was to find a different, but still focused and relatively simple, conception of the subject of the field.

The present essay presents my answer to that problem. It also implies that there are two fundamental sciences of society: that dealing with symmetrical social relationships and that dealing with asymmetric ones in social units—"economics" and "politics." Between these, a comprehensive science of social interaction must build bridges, of course, as must many particular explanations of particular social occurrences. Since the science of symmetrical interaction is well-developed (and suitable to what are generally considered "hard" methods of thought and inquiry) there has been a tendency recently to develop "economic" theories applicable to political phenomena. This is a useful tendency. But it would be more useful still if a concomitant effort were made to develop "political" theories of economics—and of politics.

National and Local Forces in State Politics: The Implications of Multi-Level Policy Analysis

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American national and state governments influence each other within a federal arrangement. This process often dominates state politics but is largely ignored in the study of states and their policies. The first part of this article points up some of the deficiencies in the current knowledge of states which stem from the ignoring of federalism; the second part suggests a refocusing of our studies around the federal arrangement.

Level Problems in the Study of State Politics

The variety of levels of American governments presents some difficulties in studying any one level, especially the state level. Substantively, these difficulties revolve around the political processes associated with the relationships between governments. Technical problems derive from the generic difficulties of level-of-analysis alternatives, of which the most familiar are ecological fallacies. For the main elements of the contemporary study of state politics, the level difficulties are considerable.

A noticeable quirk of most state studies is the treatment of states and only states as policy units. For almost any state policy, quite a number of influences go into and out of policy, and it is doubtful that most of these can be attributed to "states." For instance, most highway building and welfare giving involves the federal government's determining basic parameters and local agents' influencing the applied policy; neither of these factors can be treated in a state-only analysis of the policy, though the actions of state governments are not particularly meaningful abstracted from federal-state-local interaction.

The use of states as units implies that some attribute of the state determines or causes policy, or at least policy differences, among the states. It is not enough that the policy merely "pass through" the state or that state differences are statistically related to policy. The "necessary condition" of having states or state differences for policy does not make states the determinants of policy. If Lyndon Johnson as president pours federal money into Texas or a Washington bureau decides to allocate grants-in-aid on a per capita basis, state policy differences cannot legitimately be traced to the states themselves. Because less than one-fifth of the money passing through the hands of state governments starts and stays at the state level, state differences are *ceteris paribus* not likely to be tied mainly to state governments. Conclusions about policy or even about state policy based on a simple state-level of analysis have a substantial chance of being wrong.

Furthermore, the type of systems model used in state analysis often interacts with the single-level approach to produce a problem of spurious correlation. The systems model usually examines the interrelations of some subset of past government policy, social and economic parameters, political representation processes, bureaucratic attributes, policy, and policy impact. The beginning assumption is that these factors are independent; actually, nonstate-level phenomena often cause several of the factors at once. The proposed revenue-sharing act, for instance, includes a "tax effort" provision, so that the act would affect both state tax law and state expenditures, producing a correlation which may depend on federal, not state, policy. Poverty funds allocated on a "per poor person" basis have been shown to produce a relationship between state income and state welfare expenditures.[1] Similarly, the lack of relationships can be traced to nonstate factors, as when the federal courts stop states from correlating the black portion of their population with low election turnout.

The systems approach can introduce Galton's problem into relationships: diffusion produces similarity on many variables but there is really only one event occurring—the diffusion itself.[2] Walker found a host of socioeconomic variables reflecting perhaps one or two actually different factors—related to 88 policies (largely reflecting a single policy innovation factor) for 48 states, which for policy purposes were equivalent to five regions.[3] The thousands of relations among these variables strongly reflect a single relation of five items—the association of region and innovation.

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[1] See Andrew T. Cowart, "Anti-Poverty Expenditures in the American States," in *State and Urban Politics, Readings in Comparative Public Policy*, ed. Richard I. Hofferbert and Ira Sharkansky (Boston: Little, Brown, 1971), p. 415.

[2] For a concise discussion of this problem, see Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970), pp. 51–53.

[3] Jack L. Walker, "The Diffusion of Innovations Among the American States," in Hofferbert and Sharkansky, pp. 377–412; a somewhat revised version of this

The diffusion process itself complicates the understanding of relationships. Nearly 100 national and regional associations of governmental officials diffuse not only policies but also relations between policy and other variables, as between population and form of local government, between taxes and economic structure, and between population density and fire prevention policy. It is usually unclear whether strong relations would exist without the associations, without other state governments as pacesetters, or without bureaucrats oriented to out-of-state events, but it appears that interstate correlations depend on interstate communication in addition to the within-state factors presented in the systems approach.

The information employed in a state analysis characteristically is a single parameter for a single variable. That is, a single score represents the state for each item. Thus, AFDC welfare expenditures in a state, which comprise innumerable payments more or less closely conforming to a multitude of rules, are often indicated by the average payment per recipient. It is rarely clear whether this score is a policy, if so, a state policy, if so, indicative of the entire AFDC state policy. There is considerable room for argument over what constitutes a policy: the rules, the application of the rules, the government actions, the intended consequences, etc. The payments are measured from actions; because in different states the same rules can lead to different actions or different rules to the same actions, the payments do not indicate directly the policy rules, nor would the rules measure policy actions. If analysis is limited to policy actions, it is not always clear that the actions can be attributed to the states. Local governments do most of the welfare paying, for instance, so that the measure of state policy might be best described as the average payment by the average local government. The averages themselves may be misleading. Relatively little policy action consists in paying the average amount, and the average is indicative of all action only when the distribution of actions around the average is known and is the same everywhere. The within-state distributions are rarely reported or even calculated; where they have been reported, the distributions vary with the policy and differ considerably among the states.[4] For example, the county average monthly general assistance payments vary around the state average 75 times as much in Massachusetts as in South Carolina, while the state averages show only about 6 per cent as much policy difference. The averages do not index the policy, and the averages may not be the important source of policy differences.

The type of score representing state policy presents statistical problems. In one major study, 88 of 94 variables used were means, percentages, per capita, or otherwise severely altered scores, most commonly derived by dividing one variable by another.[5] Statisticians are leery of using such scores in a complex analysis, especially one involving powers, roots, and differences, as state policy analyses normally do. The intention of the alteration is to control for some influences, such as size of population, in order to examine other relations more clearly. The division is an inappropriate control: standard least-squares regression should be used, and it should be used on *all* the variables influenced, not just a select few.[6]

The general result of using automatic controls for the impact of population size on policy without a discussion of that impact is to hide a major source of policy differences. California and New York, for instance, are conceded by state administrators to be the primary sources of diffusion, providing models for over one-third of the important, new, and better state

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article, found in *Politics in the American States,* ed. Herbert Jacob and Kenneth N. Vines, 2nd ed. (Boston: Little, Brown, 1971), pp. 354–387, indicates (Tables 7, 8, 9) that about half the diffusion depends on four states, one in each of four innovation regions, which provide innovations to states. It would appear, though it is not entirely clear, that California and New York both provide policy models for virtually all other states, that the regionalism largely occurs in accepting innovation from what might be called second-order innovators—Wisconsin, Florida, Illinois—and in the sequence of diffusion (rather than the ultimate source of innovation).

[4] See Sharkansky and Hofferbert, "Dimensions of State Policy," in Jacob and Vines, p. 346, and Sharkansky, "State Administrators in the Political Process," in Jacob and Vines, p. 266.

[5] Thomas R. Dye, *Politics, Economics, and the Public* (Chicago: Rand McNally, 1966), pp. 303–309.

[6] It is standard statistical practice to control for the influence of a third variable on both variables in a relationship. Controlling for effects on just one of the variables produces anomalies, especially in the correlation coefficients. Controls reduce the variance in the "dependent" and "independent" variables by the amount of covariance (weighed) with the "control" variable. The reduction in the variances properly adjusts the base—the estimated maximum covariance—against which the independent-dependent covariance is measured in a correlation coefficient. When only one of the variables is controlled, only one of the variances is properly reduced, leaving an established maximum covariance which is artificially high, so the correlation coefficient is estimated to be lower than it is.

programs.[7] The reason for their policy leadership lies first in the simple scale of government: they are the largest states. While other forces also influence innovation and diffusion, any explanation leaving out the effects of population size on scale of government and thus on innovation and the other variables is inadequate. Using a population-based percentage measure, as in percentage urban, is not a good control for population: large states are disproportionately urban. The percentage measure ends up partly measuring population size, and the interpretation of its relation with policy becomes difficult. When other variables are not controlled for population—the favored value-added-by-manufacturing is virtually never controlled for population although it is an excellent correlate—population size is reintroduced into analysis without being understood as a size effect, again obscuring interpretation.

Some state analyses, while conceding much of the above, argue that important policy differences among the states can be explained with a state-only systems approach and per capita types of measures. States certainly differ on some policies, but it is not immediately clear that these differences are large or important. One straightforward method of examining the magnitude of state policy differences is to compare them with the differences for the entire U.S. population. The population as a whole varies on policies, such as the amount of state and local taxes paid by persons, and this variation can be measured by the conventional variance formula from statistics. The population variance can then be partitioned into two parts: differences among individuals within a state (deviation from the state mean) and differences among states (deviation of the state average from the national mean). The partitioning is done by a standard one-way analysis of variance formula commonly employed in psychology and sociology and available in standard statistical textbooks. The results of the analysis of variance are directly equivalent to a multiple regression using dummy variables (each state is a variable): the variation among states is the variance "explained" by state differences, the variation within states is "unexplained." The states' share of the total variance is directly equivalent to the multiple R^2 (proportion of variance explained, measured by "between sum of squares" divided by "total sum of squares") using fifty independent variables. Table 1 shows the share of the national population variance on typical policies which is accounted for by differences among the states. If anything, the table overrepresents the state share of the variance, yet states are not the locus of the main national policy differences. While ultimately political science ought to explain nearly all policy, a sense of priorities should lead us to deal first with the 98–100 per cent of the policy variance which is not accounted for by state differences. Similarly, statistical theory and practice indicate that we can have little confidence in our variables that account for only 1 per cent or even 10 per cent of the variance.

Table 1. State Share of National Policy Variance, Selected Policies, One-Way Analysis of Variance

Policy	Proportion of Variance Explained By States, in Percentages
Selective Service Exam Failures, per examinee[a]	2.13%[b]
Car Registrations, per capita	1.47%
State and Local Taxes, per capita	2.34%
State and Local Employees, per capita	0.10%
Divorces Granted, per capita	0.11%
Policemen, per capita	0.02%
Old Age Assistance Recipients, per capita	0.74%
AFDC Recipients, per capita	0.27%
Aid to the Blind Recipients, per capita	0.01%
Disabled Assistance Recipients, per capita	0.12%
General Assistance Recipients, per capita	0.12%
Unemployment Compensation Recipients, per capita	0.11%

[a] The data sources for Tables 1, 2 and 3 are *The Statistical Abstract of the U.S.*, prepared by the Bureau of the Census, U.S. Department of Commerce, 91st and 93rd editions (New York: Grosset & Dunlap, 1970 and 1972). The number of cases ranges from 590,000 selective service examinees to over 200 million total U.S. population.

[b] Entry indicates that .0213 (1/48) of national policy differences are reflected in state average policy differences.

On none of the policies do states come close to accounting for even 5 per cent of the policy action differences. The differences among states are not large;[8] if the differences are to be important, then they must have consequences out of proportion to their size. Table 2 indicates state differences as a proportion of national differences, for some measures of policy consequences. The impacts are indeed out of proportion to the policy differences: they are even smaller. The small state policy differences correspond to virtually invisible policy consequences differences.

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[7] Walker, "Diffusion of Innovations," p. 384.

[8] A number of authors use the coefficient of variation to establish that there are large differences among states. The statistic is not convincing. The coefficient of variation is meaningful only with a normal distribution and a true zero point, in which case the coefficient of variation for the American population of 200 million (where the lowest case is expected to be over 5.6 standard deviations from the mean) should be less than .18 at the maximum, when the lowest case is zero; if the lowest case is more than zero, then the coefficient of variation should decrease. The coefficient of variation for fifty states, however, is expected to have a maximum

Table 2. State Share of National Policy Consequences, Selected Variables, One-way ANOVA

Policy Impact	Proportion of Variance Explained, in Per cent Average
Infant Deaths, per live births	0.02%[a]
Rape, per capita	0.00%
Doctors, per capita	0.01%
Hospital Beds, per capita	0.03%
Mental Hospital Patients, per capita	0.03%

[a] Entry indicates that .0002 (1/50 of 1%) of national differences are reflected in state average differences.

It is less than clear that the policy impact variable differences can be easily traced to the policy differences. For instance, while policemen and incarceration are supposed to reduce rape, they all increase together, indicating that the "impact" has more influence on the policy than vice versa. The health impacts, while interrelated, are not strongly tied to state policies. Dye finds little or no relation between differences in educational policy action and dropout rates or mental failures.[9]

In comparison to policy variation, we find that social, economic, and representational differences abound in the states, as indicated in Table 3. State differences in these background factors exceed differences on any policy or impact variable. Several caveats are in order, however. On none of the variables is 10 per cent of the national variance explained by state differences, so states are hardly the key to American social organization. Further, Galton's problem reappears because states' scores are not independent of each other. About half or more of the state variance in industrialization, education, urbanization, percentage black, etc., traces to the census bureau's nine geographical regions. Indeed, socioeconomic and political scores allow a near perfect prediction of what region a state is in.[10] Of course, the differences within states—at, say, the county level—are also considerable.

Table 3. State Share of National Social, Economic, and Representational Variance, One-way ANOVA, selected variables

Variable	Proportion of Variance Explained, in Percentages
Urbanization, per capita	7.65%
Black population, per capita	7.56%
Industrial occupations, per employee	4.32%
Income, dollars per family	2.48%
Education, years per person over 25 years	2.46%
Congressional Turnout, per potential voter	4.28%
Congressional Democratic votes, per voter	2.91%

Almost all major state policy studies use measures of association and relationship based on manipulations of Pearsonian product-moment correlations. In view of the great similarity on policy among states, it is unfortunate that correlations do not indicate the sources of similarity. Rather, correlations assume *a priori* that states should be identical, and they attempt to explain violations of the assumptions. Using correlations in a state-level analysis, there is no simple way to check whether similarities are greater than expected. If the major forces involved in state policy are forces which produce similar policies across states, then the standard correlational analysis can seriously mislead our explanations of policy. The national government, functional associations of state officials, and institutionalized policy diffusion patterns among the states are but some of the forces operating to produce similar policies in the states; correlational analysis does not illuminate their policy role. The magnitude of that role can be partly estimated by the difference between the states' share of national social, economic, and political differences (Table 3) and the states' share of policy differences (Table 1). If states were even random groupings, we would expect the variance shares to be about equal; if the

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of .429 (the lowest case is expected to be about 2.33 standard deviations from the mean). We might expect, then, to find fairly high coefficients of variation in the states. The coefficient of variation is useful only as a scale device for comparing standard deviations on variables in which everything is similar except the magnitude of the scores. Any other type of difference (number of cases, modality of cases, absence of a ratio scale, etc.) will make coefficients of variation noncomparable.

[9] Dye, *Politics, Economics and the Public,* p. 98.

[10] Robert B. Dishman and Robert F. Craig, "Regionalism, State Policy Outputs, and General Explanatory Models," a paper delivered at the Annual Meeting of the American Political Science Association, New York, September 2–6, 1969; tables between pages 27 and 28.

states are primarily political groupings producing different policies, we would expect the policy variance to be considerably greater than the social and economic variance.[11] Because the policy variance among states is much less than that which could be expected from the states' social, economic, and representational diversity, the major task of state policy analysis should be to explain the *similarities* among state policies; correlations are not easily applied to this task.

The combination of individual level data (education, income) and aggregate or macro-level analysis (using states) commonly leads to serious level problems.[12] Often, strong forces at the individual level influence our estimates of higher level relationships. With only a macro-analysis, it is extremely difficult to avoid this contamination. The best estimates of the effects of contamination depend on an understanding of the relationship of the aggregate units to the variables.[13]

Though the grouping of individuals into state units is not an intentional act of analysis, the effects of the grouping are the same regardless of why or by whom it is done. The appropriate universe to measure these effects is the set of relationships at the individual level across the country. If there is a state-level set of effects, it will show up in the total individual relationship in a manner first specified by Robinson and since elaborated.[14] The impact of state-level effects depends on the share of the total variance occurring at the state level, relationships at the state level, and "aggregation bias" relationships across levels which depend on the grouping.[15] All three of these may vary considerably with the grouping method. As Blalock has pointed out, "in shifting to larger units we affect the amount of variation in all variables,"[16] and "contiguity is often the major criterion [for grouping]. But such combinations may . . . maximize variations in either the independent or dependent variables. If so, the slopes and correlation coefficients will be affected correspondingly." Aggregation bias disappears only with exact equations of one sort or another, which are not the rule in empirical political science.[17] Our conclusions about states as policy units depend on the type of grouping which states represent for policy-relevant variables.

Grouping by states minimizes variance on the dependent variable—policy itself. This is clear both from the absolute share of national variance which occurs between states (Table 1) and from the relative share of national policy compared to social, economic, and representational variance (Table 1 vs. Table 3). Policies are the same across states: they differ almost entirely within states.

For an example, we take the main business of both state and local governments: formal education, through attendance at certified schools. Here the federal government has little control, while states explicitly control education in the localities. If states have a significant policy role, it should show up in education. State differences in the proportion of 5- to 34-year-olds enrolled in school in 1960 account for 0.38 per cent of the national variance. In the age group where the national variance is the least (9-year-olds), state differences account for 0.19 per cent of the national variance. These policy differences are far less than the states' differences on some relevant social variables: states account for 1.42 per cent of the national variance in age and nearly 7.65 per cent of the national variance in urbanization. For all practical purposes, the explanation of the state policies is accomplished by a single national explanation, as for instance in Louisiana, where the enrollment rates for eight age-blocs mirror ($r = .994$) the national rates. If the purpose of state analysis is to examine national or universal patterns, then a single within-state analysis is more representative than a between-state analysis; if the purpose is merely to examine state differences, then the study is trivial. For both purposes, the between-state results are likely to be unreliable.

In using states and grouping so as to minimize variance for policy, the relationship of other variables to policy is changed. Particularly, in minimizing policy variance, the use of states as units controls for most of the valid causes of policy differences, while expanding the scope for random and error disturbances. One signal that this is occurring is that as we move from the individual to the state level, we observe a greater reduction of variance in policy than in the variables supposed to produce state policy. If a constellation of variables largely determines policy, a control for policy should control for the portion of the variance in other variables which is reliably related to policy; the remaining variance on these "independent" items should be unrelated to policy and should be small if the variable is a good predictor of policy. For example, urbanization typically implies greater school enrollment for the national sample; in aggregating by age and state, the valid relationship between urbanization and enrollment disappears, leaving only a small and unreliable (r = .003) correspondence. Similarly, age—a good negative predictor of enrollment rates (it is mostly young people who go to school), becomes a weak, positive (r = .118) correlate of enrollment when ages and states are aggregated. Because the policy variances involved are so small, however, the aggregate relationship may easily appear strong owing to random influences, as for instance in the strong positive relationship (r = .454) of age and 9-year-old school attendance at the state level. Explaining this spurious correlation is neither a major nor a rewarding task. "In grouping by Y [policy], we are confounding the effects of all these variables, this being the very opposite of the direction in which we usually wish to move," according to Blalock.[18] Almost no variable strongly related to policy remains a good predictor of policy when grouping by states because most of the valid covariance disappears, and the remaining covariance is confounded with nuisance variables irregularly related to policy. Variables which are but randomly related to policy increase considerably in potential importance, and a constellation of such variables, randomly related to policy but strongly related to each other, may easily show a series of high correlations with policy at the state level without being in any way truly related to policy. In shifting from a universe of 200 million cases to a group of 50 cases, we invite error, an effect magnified many times when the 50 cases are virtually identical on the dependent variable.

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[11] Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964), pp. 103–114.

[12] The literature on level fallacies is considerable. For a basic discussion, see Hayward R. Alker, Jr., "A Typology of Ecological Fallacies," in *Quantitative Ecological Analysis in the Social Sciences,* ed. Mattei Dogan and Stein Rokkan (Cambridge, Mass.: The MIT Press, 1969), pp. 69–86.

[13] For summary discussions, see Blalock, *Causal Inferences,* pp. 180–181 and Michael T. Hannan, "Problems of Aggregation," in *Causal Models in the Social Sciences,* ed. Hubert M. Blalock, Jr. (Chicago, Aldine, Atherton, 1971), pp. 491–496.

[14] Hannan, "Problems of Aggregation," pp. 487–489, 496–505.

[15] *Ibid.,* pp. 499–500.

[16] *Causal Inferences,* p. 180.

[17] Hannan, p. 501 and note.

The relevance of correlation coefficients in the analysis of policy at the state level is virtually nil: "Correlation coefficients merely measure the proportion of unexplained variation, which is a function of the degree to which outside variables vary as compared with variation in the independent variable,"[19] a comparison not sensible when the independent variable scarcely varies. Some analysts have argued, however, that half of the slopes of the regression may be reliable for areal groupings such as states if the grouping effect is known.[20] Generally, this is true only for simple cases; when the number of variables and relations exceeds three, and when grouping affects all relations and variables somewhat differently—the normal situation for state policy analysis—the regressions are not likely to be reliable; also, if there is a true system factor at work, its effects can be hopelessly confounded with individual level factors.

The Federal Impact on State Politics

The difficulties discussed in the previous section involved in ignoring level phenomena when studying state politics are so considerable as to suggest not only that level factors need be included in the analysis but also that the salient characteristics of state politics derive from the level arrangements. The study of state politics might well be organized around the federal relation.

Consider elections. State majorities are represented in U.S. Senate and gubernatorial outcomes; to the degree that national party rather than state is the important political dimension, this system can seriously misrepresent political opinion in the country. For instance, in the 1968 gubernatorial elections, Democrats with 51 per cent of the national vote won only 38 per cent of the offices; only 54 per cent of the voters were governed by a man for whom they had voted; while 66 per cent of the Republican voters had a Republican governor, only 43 per cent of the Democrats had Democratic governors. If the election priorities were reversed, with parties being represented first and states second,[21] overall voter satisfaction would decrease less than one-tenth of one per cent, but party satisfaction would change to 48 per cent of the Democrats satisfied and 60 per cent of the Republicans satisfied. But states do not correspond to a major dimension of voter interest —namely, parties; therefore, elections can seriously misrepresent input through malapportionment.

States as policy-making bodies are justified largely by unique local problems or interests, which a theory supporting both majority rule and minority rights might suggest should lead to different state policies based on differing state interests. Frank Munger's study of public policy and public opinion in the states throws light on this justification.[22] For a series of issues, Munger measured both the majority public opinion and the law two years later for each state and each issue. These data, when aggregated, yield two additional measures: the national law (the majority of state laws) and the national opinion (majority of state opinions) for each issue. Munger's original measures show state democracy when a state's laws match its citizens' opinions. The additional measures allow us to examine the relations between a state's opinion and the national opinion, between a state's law and the national law, and between national opinion and national law. When states differ from the national pattern on both opinion and law, we might infer that they are working democratically to accommodate unique state interests. Only on three questions —segregation, aid to private schools, and right-to-work laws—do state interests produce state policy in a democratic manner. On these questions, 39 per cent of the states have publics which, in the majority, disagree with majority national opinion, and 37 per cent of the states have laws which disagree with majority of state laws; state opinion is matched by state law 71 per cent of the time, which is 20 per cent more than expected from the marginals; thus, while the translation of opinion into law is considerably less than perfect, it does approximate democracy.[23] These issues tap directly into the major political symbols of national racial, religious, and class division. The federal government prohibited states from segregating by state law, strictly regulated where it has not prohibited state aid to private schools and encouraged state right-to-work splits as a tactical ploy in a national party conflict. The ability of states to legislate democratically on the basis of state interests on fundamental issues depends on the national government.

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[18] *Causal Inferences,* p. 108.

[19] *Ibid.,* p. 180.

[20] *Ibid.,* p. 180.

[21] Under such a scheme, the party share of offices would be decided by the party share (to the nearest even percentage) of the national vote, with the *N* Democratic governors going to the *N* most Democratic states.

The federal role in the fundamental issues is critical because these are the only issues on which states divide or approximate democracy. On 53 other questions, state public opinions agree with national opinion more than 90 per cent of the time, state laws agree with each other 87 per cent of the time, state opinion agrees with state law 57 per cent of the time, which exceeds the agreement expected from the marginals by less than 1 per cent.[24] On these issues—divorce, gun control, abortion, police strikes, gambling, etc.—only ¼ of 1 state can characteristically be considered democratic. Slightly more democracy is produced by states following national trends rather than within-state events (state opinion agreeing with national opinion and state law diffusing from other states). Contranational state public opinion corresponds to state law only 37 per cent of the time—well below chance (which is 50 per cent for these dichotomous issues). States are controlled by the federal government on divisive issues and are not operationally democratic on other issues. Democracy in the states would be considerably improved if policy making were nationalized (by passing a uniform law whenever ¾ of the states' statutes agree), so the states are not particularly effective even at diffusing national trends democratically.[25] Autonomous states do not protect unique state publics from disagreeable national trends; to the degree states differ from substantial national trends, they tend to frustrate their citizens.

In spite of their critical constitutional position, states are not well received by the public, which pays less attention to them and trusts them less than other levels of government. By a considerable margin, citizens think they get less for their tax dollars from states than from other levels and that for comparable policies states are more unfair.[26] In their politics, citizens are nationally and locally oriented. Party identification, for instance, is national. For House of Representative elections in the 'fifties, states characteristically contributed only 1½ to 2 per cent variation in turnout and party vote.[27] With any disconnecting of state politics from national politics—through nonpartisan or odd-year elections, for instance—citizen involvement and understanding tend to drop drastically. For the governed, states have a secondary and largely derivative role.

The increasing gap between the nominal constitutional role and the actual role of the states poses some problems for traditional justification of state politics. The nationalizing of politics includes an individualizing of politics and an increased role for non-areal groups, while diminishing the significance of states. The Democratic party has decreed that individual Democrats, not areal party organizations, comprise the national party and should get represented at national conventions. The increased orientation to policy for voters and parties increases the party role while decreasing areal factors.[28] Public opinion differs little among the states, and so do laws. In this situation, the traditional argument for states as representatives of local interests and democratic units carries little weight.

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[22] Frank Munger, "Opinions, Elections, Parties, and Policies," a paper delivered at the Annual Meeting of the American Political Science Association, New York, September 2–6, 1969.

[23] Munger, from Table 1, pp. 7–12. The marginals used here to generate expectations are those for an entire class of questions. Results are similar if marginal expectations are generated separately for each question.

[24] *Ibid.*, Table 1.

[25] The uniform national law, while improving opinion-law democracy, removes the possibility of further innovation on that policy. Although a variety of mechanisms (such as time limitations on the uniform law) could remove this disability, the whole suggestion is an academic standard rather than a practical proposal.

[26] "Public Opinion and Taxes," United States Advisory Commission on Inter-governmental Relations, (Washington, May, 1972), Table A-3, p. 15, Table A-1, p. 13, Table A-4, p. 16.

[27] Expected deviation, calculated from the state variance components reported in Donald E. Stokes, "Parties and the Nationalization of Electoral Forces," in *The American Party Systems*, ed. William Nisbet and Walter Dean Burnham (London: Oxford University Press, 1967), Table 1, p. 186.

Across time, states have been diminishing in their importance and diversity. Table 4 presents the average standard deviations for states in partisanship in the presidential elections. States have declined as locales for national political controversy.

Table 4. Average Standard Deviations of Presidential Percentages of Popular Vote in States, 1900–1960*

Decade Midpoint	Average Standard Deviation
1900	16.8
1910	15.3
1920	15.2
1930	13.7
1940	12.7
1950	9.1
1960	8.1

* Adapted from Frank Sorauf, *Party Politics in America*, 2nd ed. (Boston: Little, Brown, 1972), p. 52.

Hofferbert and Sharkansky report steadily decreasing socioeconomic and policy variation among states since 1890 and indicate that the connection between these factors has been weak, as with the correlation between per capita income and state and local spending, which decreased from .93 in 1903 to .56 in 1964–5.[29] Although the differences diminish, the states largely maintain their respective positions; for instance, the rank-order correlation between Hofferbert's industrialization factor in 1890 and in 1960 is .72.[30] Similarly, state policy innovation patterns during the long period of Republican dominance are largely the same as during the current period, though total policy differences among states have been diminishing.[31] Thus, the nationalization of American society has not particularly implied any major redistribution among states.

The increasing similarity among states over time runs counter to several common models of integration. Changes are not randomly distributed around earlier differences, so the process of change is directional. The similarity does not follow a model of clustering and complementary location which individuals, corporations, etc., left to their own devices tend to follow. Nor do the changes follow the trends of state policy differences, which on the whole would lead to increased dissimilarity over time. The increasing similarity most closely resembles the pattern which would be produced by strong, uniform policies, either in the form of national policies or in the form of similar state policies. The similar state policies could derive from the imposition of national norms or from cooperation and imitation among states.[32] The policy similarity cannot derive from similar internal policy-making politics, for these would decrease the resulting similarity. Thus the broad trend is for national patterns to override state differences in policy making and the consequence is increasing similarity among states in the effects of policy.

The formal similarities between state and federal governments are misleading. Despite the nominal commonalities including single chief executive, bicameral legislature, plural-member supreme courts, formal constitutions, Democratic and Republican party cleavages, formal areal representation, civil service, and the obvious similarities in political culture and socioeconomic environment, states do not resemble the federal government in their actual operation. Indeed, for all the formal dissimilarities, local governments are closer to the national government than are state governments. Primarily, states cannot usefully be viewed as political systems (or economic systems or social systems), while the nation can be.[33] Briefly, this means that virtually all parts of state politics are more closely connected to factors outside the state than to other political elements within the state. States are collections, on an areal basis, of assorted subsystems of national and local politics. The preceding presentation of data on social, economic, representational, and policy patterns illustrates the close connection of states to a national pattern. The connections within states are weaker and more variable, partly of necessity—what in Louisiana could be correlated with school enrollment rates more than $r = .994$, which is the connection with national enrollment rates? The correlation of state "industrialization" with state Democratic presidential vote varies from $-.38$ to .35 during the period 1890–1960, for instance.[34] Mostly, the correlation of items for states follows the national correspondence. State policies are not, therefore, the "outputs" of a state system with state "inputs." Rather, state policies are mainly the product of national policy trends, between-state communications and imitations, local within-state interests, and some state level political and socioeconomic considerations.

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[28] For discussions of the changes over time in voters' orientations to parties, see John C. Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science*, 14 (February, 1970) 25–42; David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400; John O. Field and Ronald E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *The Public Opinion Quarterly*, 33 (Fall, 1969), 380–398; and Gerald M. Pomper, "From Confusion to Clarity," *American Political Science Review*, 66 (June, 1972), 415–428.

[29] "The Nationalization of State Politics," in *State and Urban Politics*, pp. 469, 472 and "Dimensions of State Policy," p. 322.

[30] "Socioeconomic Dimensions in the American States," in *State and Urban Politics*, pp. 456–457.

[31] Walker, "Diffusion of Innovations," pp. 360–361, and column 1, Table 6, p. 377.

[32] Competition among states might also be a source of convergence. The states are unequal competitors, however, so highly similar policies might not result from competition.

[33] For a similar suggestion, see Herbert Jacob and

Any adequate model of state policy making cannot build in the predominance of state forces. Rather, the importance of state forces is one of the main characteristics of a state or of a policy. For instance, the highest correlation ($r = .425$) of state democracy for Munger's data is with states' absolute deviation from the average innovations pattern. That is, states which are democratic *either* lead in innovating or refuse to follow policy diffusion; in both cases the rejection of the national state policy norm produces more cohesion between policy and opinion within the state. For particular policies, national norms may allow more or less room for state influence. Differences among states' school enrollment rates for five-, six-, and nine-year-olds account for 12.1 per cent, 7.1 per cent, and 0.2 per cent, respectively, of the national variance, so national norms are less strong as an influence on states' policies for the younger age groups. For the states with the lowest overall enrollment (Alaska) and the highest (Utah), the 5- to 16-year-old age *deviations* are strongly correlated ($r = .80, .79$) with the level of national norms; states differ most in the tangential aspects of enrollment policy and differ little when enrollment is critical. Though deviations for the most variable enrollment group—five-year-olds—are related to state characteristics such as innovation and urbanization ($r = .62, .56$), these relations largely occur as a result of between-state interaction; for instance, 73 per cent of the enrollment-innovation relation results from the six state regional innovation patterns, and 67 per cent matches simple geographical contiguity as indicated by the Census Bureau's four regions. Most of the remaining relation between urbanization and enrollment validly indicates that, within states, urban areas have a higher enrollment rate for six age groups than do rural areas, a relatively uniform national pattern. The state policies thus depend on a variety of levels of influences, as well as a variety of influences within levels. There might well be a general zero-sum relationship between the impact of national forces and every other influence.

A final salient characteristic of states is the existence of two separate major dimensions in the politics of policy making. Analysts differ on the exact nature of these dimensions. Hofferbert and Sharkansky suggest that they consist of citizen representation ("competition-turnout") on the one hand and bureaucratic performance ("professionalism–local reliance") on the other hand.[35] The dimensions involve different demographic factors and operate largely on different policy areas.[36] Somewhat equivalently, Walker's innovation and Munger's opinion-law democracy are major, separate policy-making dimensions which suggest a bureaucratic and a citizen representation factor. While the analyses are not completely comparable, both suggest a largely "internal" and a largely "external" dimension operating in policy making, each tied to its own sector and reflecting different, rather autonomous, national forces.

Some of the consequences of the dimensions are suggested by a detailed study of four states.[37] Munger's opinion democracy relates directly to the policy accountability of incumbents in elections, and Walker's innovation scores match the behavioral index for governmental legitimacy (see Table 5). Innovation seems to produce legitimacy (or perhaps legitimacy allows faster diffusion), while electoral accountability for policy seems to produce opinion-law democracy.[38] As with innovation and democracy, policy accountability and government le-

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Michael Lipsky, "Outputs, Structure, and Power," in *State and Urban Politics*, p. 21.

[34] Hofferbert, "Socioeconomic Dimensions in the American States," Table 6, p. 460.

[35] Sharkansky and Hofferbert, "Dimensions of State Policy," pp. 331–333.

[36] *Ibid.*, pp. 338–339.

[37] Douglas Rose, "State Election Systems," (Ph.D. dissertation, University of Minnesota, 1970), chapters 4 and 5.

[38] Accomplished mainly through strong party organization and at the expense of individual demogogic and coalition-building skills. See Rose, "State Election Systems," Chapter 5.

Table 5. Innovation, Legitimacy, Democracy and Accountability in Four States

State	Innovation[a]	Legitimacy[c]	Democracy[d]	Policy Accountability[c]
Oregon	.544[b]	1.5	.621	1.1
Wisconsin	.532	1.1	.517	.4
Vermont	.414	.0	.509	−.3
Mississippi	.298	−.4	.621	.7

[a] From J. Walker, "Innovation in State Politics," in *Politics in the American States*, ed. Jacob and Vines, 2nd ed. (Boston: Little, Brown, 1971), p. 358.

[b] All scores have true zero points; innovation and democracy can reach a maximum of 1.0; legitimacy and accountability absolute scores do not have a precise interpretation outside the state voting context, but a score of 1.0 would indicate that about 1 per cent of a state's eligible voters will turn out and cast their ballots as if voting on this factor alone.

[c] From D. Rose, "State Election Systems," (Ph.D. dissertation, University of Minnesota, 1970), pp. 238, 242, 247, 251.

[d] From F. Munger, "Opinions, Elections, Parties, and Policies," delivered at the 65th (1969) Annual Meeting of the American Political Science Association, p. 18.

gitimacy are linearly unrelated, but increases in policy accountability produce deviant legitimacy scores. That is, accountability and democracy lead away from the normal pattern of mild innovation and mild legitimacy.

The striking characteristics of state politics appear to derive from level effects. The level effects may produce interaction within states, leading to some broad differences in the types of politics occurring in different states.

The basic elements of state politics from constitutions to family structure to traffic laws are national, both in the sense of being fairly uniform across states and in the sense of national change occurring. These national changes do not result from similar intrastate processes but rather occur with similar interstate and state-nation processes. The basic pattern for state politics is for each element to be largely unrelated to any other element except through their relations with national patterns, which themselves may be causally connected. States are not political systems, though they are parts of a political system.

Within this basic structure, state departures from national patterns reflect past departures minus the relatively uniform impact of national policy. The total amount of differences across states on any element mostly indicates the impact of national policy on that element: policy itself shows small differences, while largely unregulated elements, such as urbanization or religious preference, show noticeable differences.

For most states, political activity is regular but not strenuous. The electorate selects potential national officials, holds taxes down somewhat and supports education; bureaucrats suggest and implement standard laws; legislators adopt the standard laws and cut budget requests. The connection between one activity and another is tangential or small.

As long as the national pattern is functional and the state differences from national patterns on any element are not important, or not related to differences on other elements, then states can function as rote conduits of national patterns. But if the national pattern becomes dysfunctional, then states need to become systems. The 1929 crash and the contemporary school busing issue are perhaps instances. There is little evidence that states do become systems in these instances, and there are compelling reasons why they cannot do so. States do not control their borders, or the large part of their commerce which is interstate, or their basic political arrangements, or the host of other factors which they would have to control to be political systems. It is unlikely that even if they could control these, states could institute major change in the required short period of time. The national government, of course, is unlikely to allow states to control such matters, for to do so would eradicate the national system (whose priority was more or less established by the Civil War).

State differences from the national pattern on any element are likely to be important only when the differences are large and the item is fundamental. Aid to parochial schools in Catholic states, the closed shop in industrial states, segregated schools in Southern states are illustrations of such differences, as are the severance taxes in states heavily dependent economically on mineral extraction. Because states are not normally systems, it takes an enormous amount of unified political power to get them to operate as such. Mass coercion from a relatively homogeneous electorate is likely to be a required ingredient in the application of power to produce deviant policy. Aside from the considerable difficulties involved within the state in producing deviant policy, two additional constraints operate. First, the national government may not allow the policy. Second, the basis on which the power and policy rest will, in the normal course of events, erode over time as national policy on other matters decreases dissimilarities among states. It is quite possible to have a mildly coherent state mobilization system which cannot govern in accord with the politics, or a deviant policy whose political justification has eroded, or both occurring in separate policy areas.

Interrelated state differences from the nation may produce consistent differences across a range of policies. While it is possible for a state to operate systematically to produce the consistency, this would require that the differences be of major importance. What is more likely is that marginal consistency is built in at some stage of policy making. Regional policy diffusion appears to occur because administrators take account of patterned differences; the matching of law and public opinion to produce consistent deviation tends to occur as legislators perceive consistent interrelated policy preferences (ideology) in the electorate.[39] Such consistency amounts to being above or below, faster or slower than the national pattern; it is limited by the pattern itself and by the lack of completely interrelated state differences from the nation. Because the national pattern is a moving pattern which carries states ever closer to itself, even policy deviancies based on built-in accommodation of interrelated differences are likely to be unevenly and generally decreasingly deviant.

Most states are not systems but reflectors of national patterns. Some states which regularly precede or lag behind the national pattern on many elements may behave as if they were systems in part, but the operating consistency resolves to mechanisms in the bureaucracy or representation process for adjusting the national pattern. Some states with important differences from the nation may have a systematic politics or basically deviant policies, but the system arises in response to the national pattern, and the existence of deviant policies depends on the national government. In all these cases, whether or not the state is or appears to be consistent, the national pattern is the dominant influence. The national influence operates most massively in a direct causal fashion, but even differences among the states' patterns of politics arise in reaction to state-national differences. In understanding the politics of any state, the most important information to know is the national pattern.

States whose electorates' preferences differ either consistently or importantly from the national pattern are more likely, as in Mississippi and Oregon, to exercise policy control over incumbents through elections, producing a closer-than-average (though hardly perfect) correspondence between public opinion and state law—democracy. Such states are better able to withstand national electoral trends—the pro-Democratic 1958 and 1964 forces, for instance—than are states such as Vermont and Wisconsin.[40]

The existence of a relatively coherent state political system like those in Oregon and Mississippi, based on differences from the nation, tends to produce deviant policy, reflected in Oregon's high innovation score and Mississippi's low score, as legislators and administrators either build in the preference differences or build in factors which are related to preference differences.[41] Because the systems are not complete and because national patterns continually change, neither state is thoroughly deviant. It seems likely, however, that all states are more deviant on policy than could be expected simply from the strength of the representational arrangements; this suggestion is consonant with the notion of relatively autonomous forces in administration which operate as though there were a coherent democracy.

State differences from national policy patterns have quite divergent consequences depending on their direction (i.e., whether they are ahead of or behind the national pattern). Because the national pattern over time tends to be unidirectional, the continuance of that trend requires the national government to allow states to innovate in the direction of the anticipated pattern, and this requirement is usually met. The national government allows innovative deviancies (such as segregation in the 1890s, primary elections, divorce leniency, permissive abortion, etc.) when they are first initiated and if they represent a trend which fits with the rest of national policy. State policy deviancy which lags behind the national pattern, however, eventually gets revoked or undercut by national policy. Thus the long-term policy consequences of state deviancy depend on the direction of deviancy with regard to the national pattern. States in advance of the nation are supported by national policy, and even where the policy is in advance of state citizen preferences, the effects of national patterns will eventually make the policy appropriate for the state. States which lag behind the nation get undercut by national policy, either directly or through the overtime workings of national policy impacts. It is this asymmetry which appears to produce the close relation between policy innovation and the legitimacy of state governments to their citizens.[42] Over time, either directly or indirectly, laggard states will have a high proportion of ineffective or inappropriate policies—a tendency which is likely to lower state legitimacy. Similarly, innovative states receive a legitimacy bonus over time.

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[39] Munger, "Opinions, Elections, Parties, and Policies," pp. 30–31.

[40] Rose, "State Elections Systems," p. 215, pp. 158–159.

[41] Wisconsin's innovation and progressive policies on welfare-education appear rooted in earlier state-national differences associated with the Progressive movement and continued through the state bureaucracy which is one of the major monuments of progressivism. The bureaucracy itself can continue as "too" innovative for the state because, as discussed in the text, it is highly functional for a state to be innovative. The origins of the Wisconsin progressive deviancy can speculatively be traced to the deviant "administrative" culture of the large German population, the availability of the progressive movement, the alienation from the Democratic party in World War I, and the feudal electoral politics associated with the La Follettes.

The above is a hypothesis about why most states are mildly democratic and legitimate and others are moderately democratic and illegitimate; the democracy and legitimacy are related in all states, but the relation in addition to the scores depends on the state-national contrast for a variety of elements. It should be re-emphasized that the differences among states are marginal—that is, no state is very systematic, democratic, or innovative. The forces of nationalization are so strong that they dwarf as well as create differences among states.

Summary

Studies of state politics which ignore events and relations at other levels are severely limited in their usefulness for both methodological and substantive reasons. The salient elements of state politics do not derive from relations within states, so it is necessary to examine external relations of states, especially relations to national patterns, to understand state politics. The basic dynamics of state politics may be rooted in the impact of national patterns on states and the reactions of states to those patterns.

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[42] One possibility is that the legitimacy that the national government has for a state's citizens may correspond (partially) to the legitimacy of the state government, for the same conflictual process affects both of them.

Innovation in the States: A Diffusion Study*

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In the past decade political scientists have witnessed an outpouring of literature on state policy outputs. Although policy is generally acknowledged to be a multidimensional phenomenon, comparative state policy research, with a few exceptions, has had expenditures as its primary focus. One of the few nonmonetary dimensions investigated previously was innovation by states.[1] This article seeks to extend in a more rigorous fashion the investigation of innovation by states.

Introduction

An innovation is generally defined as an idea perceived as new by an individual; the perception takes place after invention of the idea and prior to the decision to adopt or reject the new idea.[2] In this study, as in Walker's, an innovation is more specifically defined as a law which is new to the state adopting it, i.e., it is equivalent to a single adoption.

The observable data are the adoptions by states of particular laws. When states first learned of the idea is unknown. The data do not encompass new ideas or practices adopted by the state's bureaucracy, nor do they include "disadoptions" by means of a state court's declaring a law unconstitutional. In practice, innovation may stimulate huge appropriations or it may have little monetary impact if the program is adopted but never funded. Consequently, there is good reason to study the process by which states adopt new ideas as well as the process by which they maintain existing programs, i.e., expenditures.

The laws under consideration here are from issue areas central to the "have have-not" struggle, described as the essence of politics by V. O. Key, Jr.[3] It is more likely that a political explanation, not an economic one, can account for differences in selected "have-not"-oriented policy areas than it can for the broad range of policy areas included in some studies.

The policy areas selected are education, welfare, and civil rights. Education was singled out by Key as an arena of "have have-not" conflict;[4] welfare was the focus of Cnudde and McCrone's study specifically because of its centrality to the "have have-not" struggle;[5] the adoption of civil rights was the focus of a study by Lockard, one of Key's former students.[6] Within the areas selected there is some potentially interesting variation: Education is probably less "have-not"-oriented than the other two fields; civil rights innovations ordinarily would require less funding than public welfare or education programs.

Insofar as possible, the laws selected were ones whose adoption was free of federal influence because in expenditure analyses it has been found that federal spending often has a substantial impact on state and state-local spending patterns.[7] Some of the laws considered here were enacted by states prior to the federal government's entry into the field, e.g., public welfare laws before 1935. Others deal with subjects exclusively in the state's jurisdiction, e.g., teacher certification. This distinction between federal stimulation and state initiative is not made in Walker's exploratory work. Walker treats the dates on which states began to participate in federal grant-in-aid programs the same as the dates states adopted laws independently. Hence, the results of the two efforts may vary somewhat.

The laws also were selected for the durability of the issue; it took a long time for all states to adopt any one law. Indeed, the data extend back as far as the 1780s in one case. In some instances the process of adoption by all states is not yet complete. All laws in the three policy areas which meet these criteria and whose dates of adoption were summarized in available sources were selected. Table 1 lists the laws, the time periods during which they were adopted,

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* The author wishes to acknowledge with gratitude the constructive criticisms of Professor John Sprague and Professor James W. Davis, Jr., Washington University, and Professor John Wanat, University of Kentucky. The author is also grateful for the financial support of the Woodrow Wilson Foundation.

[1] Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 880–899.

[2] Everett M. Rogers, *Diffusion of Innovations* (New York: Free Press of Glencoe, 1962), p. 13.

[3] V. O. Key, Jr., *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), p. 307.

[4] Key, p. 307.

[5] Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 858–866.

[6] Duane Lockard, *Toward Equal Opportunity: A Study of State and Local Anti-Discrimination Laws* (New York: Macmillan, 1968).

[7] Richard E. Dawson and Virginia Gray, "State Welfare Policies," in *Politics in the American States*, 2nd ed.; ed. Herbert Jacob and Kenneth N. Vines (Boston; Little, Brown and Co., 1971), p. 459.

and the number of states adopting them.

Using this data base, we will consider three questions: (1) How do new ideas diffuse or spread among the states? They may diffuse like many other new ideas, through user interaction. If this diffusion process is regular, i.e., predictable, a dynamic model of the process can be constructed. Comparison can be made between policy areas to ascertain how diffusion differs according to the issue involved. (2) Why are some states more innovative than others? The common hypotheses relating competition and economic resources with public policy will be tested and the results compared by issue area. (3) Are there identifiable patterns of innovation? An effort will be made to determine if the same states are innovative in all three policy areas.

Diffusion of Innovations

The process by which an innovation spreads is called diffusion; it consists of the communication of a new idea in a social system over time.[8] Diffusion research has been carried out in the disciplines of: anthropology (e.g., the diffusion of cultural traits among primitive tribes); rural sociology (e.g., the diffusion of hybrid seed corn among farmers);[9] medical sociology (e.g., drug adoptions by physicians);[10] education (e.g., some 150 studies made at Columbia University under the direction of Paul Mort); industry (e.g., the diffusion of a new product among consumers); political science (e.g., the diffusion of city manager governments in the 48 states);[11] and medicine (e.g., the contagion of a disease).[12]

Empirical investigation in rural sociology, sociology, and education has demonstrated that for a wide variety of innovations, the frequency of their adoption over time is normally distributed; their cumulative distribution over time has the "S"-shape of the cumulative normal curve.[13] Three explanations are offered in the literature for this repeated finding:

(1) The time of adoption for any given case "is determined by the interplay of an infinitely large number of elements in the social milieu," thereby fitting the requirement of the normal distribution that "the value of each event is the

Table 1. Innovations in Three Policy Areas, Duration of Adoption Process, and Number of Adopters

Innovation	Time Period	No. of Adopters
Education		
State Boards of Education	1784–1949	40
Chief State School Officer	1835–1912	37
Compulsory School Attendance	1852–1918	48
Degree Requirement for Teaching in Elementary School	1930–1969	46
Degree Requirement for Teaching in High School	1896–1966	44
Welfare		
Merit System for State Welfare Dept.	1883–1942	48
Old Age Assistance	1923–1938	48
Aid to the Blind	1898–1945	48
Aid to Families with Dependent Children	1911–1937	48
Civil Rights		
Anti-Discrimination in Public Accommodations	1947–1966	31
Fair Housing (Public or Private)	1937–1965	19
Fair Employment	1945–1966	28

Sources: Fair Employment, Housing, Public Accommodations: Duane Lockard, *Toward Equal Opportunity: A Study of State and Local Antidiscrimination Laws* (New York: Macmillan Co., 1968), p. 24; U.S., Civil Rights Commission, *Voting*, Book 1 (Washington: Government Printing Office, 1961), pp. 208–210; U.S., Housing and Home Finance Agency, *Fair Housing Laws* (Washington: Government Printing Office, 1964), p. 10.

Merit System: Council of State Governments, *Book of the States, 1952–53*, 9 (Chicago: Council of State Governments, 1952), 179.

OAA, AB, AFDC: Anne E. Geddes, *Trends in Relief Expenditures, 1910–1935*, U.S., Works Progress Administration, Division of Social Research, Research Monograph 10 (Washington: Government Printing Office, 1937), pp. 91–92.

Degree for Elementary and High School: T. M. Stinnett, *A Manual on Certification Requirements for School Personnel in the United States* (1967 ed.; Washington: National Education Association, 1967), p. 80.

Compulsory School Attendance: August W. Steinhilber and Carol J. Sokolowski, *State Law on Compulsory Attendance*, U.S., Department of Health, Education, and Welfare, Office of Education, Circular #793 (Washington: Government Printing Office, 1966), p. 3.

Chief State School Officers and State Boards of Education: U.S., Federal Security Agency, Office of Education, *State Boards of Education and Chief State School Officers*, Bulletin #12 (Washington: Government Printing Office, 1950), p. 27 and p. 8.

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[8] Rogers, *Diffusion of Innovations*, p. 13.

[9] Bryce Ryan and Neal C. Gross, "The Diffusion of Hybrid Seed Corn in Two Iowa Communities," *Rural Sociology*, 13 (March, 1943), 15–24.

[10] James S. Coleman, Elihu Katz, and Herbert Menzel, *Medical Innovation: A Diffusion Study* (Indianapolis: Bobbs-Merrill Co., 1966).

[11] Edgar C. McVoy, "Patterns of Diffusion in the United States," *American Sociological Review*, 5 (April, 1940), 219–227.

[12] Norman T. J. Bailey, *The Mathematical Theory of Epidemics* (New York: Hafner, 1957). For sources of 1500 other diffusion studies see: Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations: A Cross Cultural Approach*, 2nd ed. (New York: Free Press of Glencoe, 1971), pp. 388–466.

[13] Rogers, *Diffusion of Innovations*, chap. 2, *passim*.

result of the chance combination of a great many minute and relatively equal factors."[14]

(2) The cumulative normal curve is similar to an individual's learning curve which is "S"-shape in its cumulative form. Adoption by a state is then equivalent to a learning trial by an individual.[15]

(3) There is an interaction effect, i.e., adopters influence those in the social system who have not yet adopted. As more persons adopt, the effect on nonadopters increases.[16] Actually, the normality or non-normality of the adopter distributions is independent of the theoretical assumption that ideas spread because adopters somehow influence nonadopters.[17] Other curves, particularly the logistic curve of population growth, have been widely used to fit the same kind of data (innovations) with the same goodness of fit.[18] The interaction explanation is more appealing on substantive grounds; observers of state governments point out that decisionmakers emulate or take cues from legislation passed by other states. Indeed, this function is institutionalized in the Council of State Governments, financed largely by states. Walker argues that this competition of ideas largely determines "the pace and direction of social and political change in the American states."[19] Thus, in formal diffusion theory and in Walker's application of it to state governments, one assumption is that gain in adoptions is due to nonadopters' emulation of adopters. In the following section a simple interaction diffusion model is developed and then evaluated using the twelve laws being studied here.

A Diffusion Model Based on Interaction. The rate of spread of adoptions can be denoted by ΔA_t and is some function of those already adopting:

(1) $\Delta A_t = f(A_t)$,

where A_t is the cumulative proportion of adopters in the tth year and ΔA_t is the difference in the cumulative proportion of adopters at t and $t+1$, defined as $\Delta A_t = A_{t+1} - A_t$.[20]

The most elementary way to consider the function f is as the number of pair relations between adopters and nonadopters or simply their product. This interpretation assumes that the population is completely intermixed, i.e., that "leaders" from each adopter state come in contact with "leaders" from each nonadopting state. Furthermore, the model omits diffusion from other sources, e.g., innovation stimulated by the minimum standards attached to federal grants-in-aid.[21]

If the proportion of adoptions is proportional to the interaction between those who have adopted and the potential adopters, then f could be expressed as:

(2) $f = bA_t(L - A_t)$,

where b is the coefficient of diffusion from interaction,

L is the maximum possible proportion of adopters for a particular law, and

$L - A_t$ is the pool of potential adopters in year t.

If $L = 1$, all states have the potential for adoption. There are compelling reasons, however, why every state will not be susceptible to adopting a particular law: Hard-to-amend limitations in the state's constitution or values of the political subculture might cause a state's leaders to be practically immune to diffusion from interaction. Therefore, L, the limit on the pool of eligible adopters, should be treated as a parameter to be estimated for each law.

By substitution,

(3) $\Delta A_t = bA_t(L - A_t)$ and, after rearranging and decrementing the index t,

(4) $A_t = A_{t-1} + bA_{t-1}(L - A_{t-1})$.

The cumulative proportion of states having adopted any law at year t depends upon the proportion of states retaining the law plus some proportion b of the interaction between previous adopters and eligible adopters. The coefficient of the first term, A_{t-1}, is set equal to 1, implying that all states which adopt a law keep it. Although this assumption is not strictly true in all cases (e.g., states may retain a law but not enforce it or states may revoke laws, as Mississippi, South Carolina, and Virginia have done for compulsory school attendance), the data sources are not systematically informative on this point. Hence, this model does not allow the process to go "backward."

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[14] H. Earl Pemberton, "The Curve of Culture Diffusion Rate," *American Sociological Review*, 1 (August, 1936), 550, 549.

[15] Rogers, p. 153.

[16] Rogers, p. 154–155.

[17] Rogers, p. 154–155.

[18] Coleman, Katz, and Menzel, pp. 100–103; however, for a warning on the futility of curve fitting as a satisfactory test of theoretical relevance see: William Feller, *An Introduction to Probability Theory and Its Applications*, II (New York: John Wiley & Sons, 1966), 52.

[19] Walker, "Diffusion of Innovations among American States," p. 890.

[20] For explanation of the application of difference equations to social data see: Samuel Goldberg, *Introduction to Difference Equations* (New York: John Wiley & Sons, 1958).

[21] More elaborate models could be constructed in which there is diffusion from a constant source like the federal government, or in which there is incomplete mixing of the population, e.g., regional or professional communication networks may produce distinctive diffusion patterns. For these more elaborate models see: James S. Coleman, *Introduction to Mathematical Sociology* (New York: Free Press of Glencoe, 1964), chap. 17.

By rearrangement, the equation becomes

$$(5)\quad A_t = 1A_{t-1} + bLA_{t-1} - bA_{t-1}^2,$$

whose parameters can be estimated by the following regression equation:

$$(6)\quad A_t = (1 + bL)A_{t-1} - bA_{t-1}^2 + c + e$$

where c is the intercept and

e is the error term.

If the intercept c is near zero, and if the error, e, meets standard expectations, then regression may furnish useful estimates of b, the contribution from interaction, and L, the maximum proportion of states susceptible to adoption of any one law.[22]

The interaction model was evaluated using as observations the cumulative proportion of states having adopted a particular law. Figure 1 below displays an example of the fit of the curve predicted by the model for adoption of the twelve laws. The squared term of the equation causes the regression line to depart from linearity and allows the "S"-shape characteristic of some cumulative adopter distributions.

The fit of the regression line is very good for all twelve laws, as Table 2 shows. Relying upon R^2 as the goodness-of-fit criterion, one can observe in column 1 that the variance explained ranges from .9380 for AFDC to .9976 for high-school degree requirement. Referring to the graphs, one will note that when adoptions are plotted over time, the high-school degree requirement data (Figure 2) approach a normal curve, but AFDC (Figure 3) presents quite a different picture—a damped oscillatory pattern.

It was stated previously that the error must be small and the regression constant c must be near zero in order for true estimates of the parameters to be given. As Column 2 of Table 2 shows, the intercept is very close to zero except in one case. That case is AFDC, whose intercept is .1320; that figure is sensible when you examine its graph (in Figure 3). The third column of Table 2 reports the error of the dependent variable relative to its standard deviation. The standard errors are rather small; again AFDC is the most aberrant. Therefore, by these criteria the simple model fits fairly well for these twelve laws.

Another basis on which one can evaluate the fruitfulness of the model is to examine the parameter estimates. Columns 4 and 5 of Table 2 allow for comparison of the estimated value for the parameter L and the last observed value. For eight of the twelve cases, the estimated value is close to the observed value; in two cases, the observed value is overestimated; in two cases, underestimation occurs. For each of the latter four cases there are technical reasons to anticipate poor predictions of L: low R^2's for

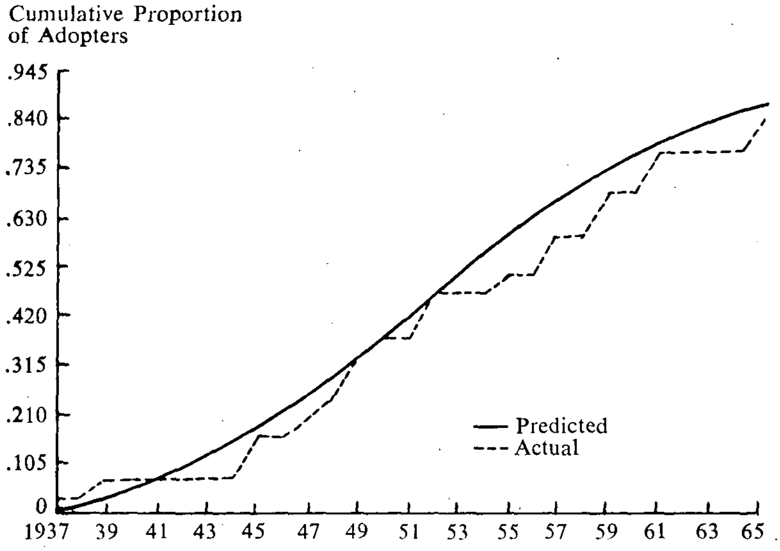

Figure 1. Predicted and actual curves for adoption of fair housing legislation, 1937–1965

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[22] J. Johnston, *Econometric Methods* (New York: McGraw-Hill Book Co., 1963), p. 7.

Table 2. Evaluation of Interaction Diffusion Model, $A_t = A_{t-1} + bA_{t-1}(L - A_{t-1})$, for 12 Innovations

Laws	(1) R^2	(2) Intercept	(3) Standard Error	(4) Estimated L^*	(5) Observed L^*	(6) b
Civil Rights						
Public Accommodations	.9756	.0250	.0337	.590	.651	.15
Fair Housing	.9841	.0068	.0172	.391	.399	.32
Fair Employment	.9615	.0278	.0341	.468	.588	.06
Welfare						
Merit System	.9453	−.0227	.0483	1.486	1.000	.22
Old Age Assistance	.9772	.0039	.0570	1.079	1.000	.38
Aid to the Blind	.9931	−.0027	.0290	1.145	1.000	.14
AFDC	.9380	.1320	.0690	.769	1.000	.08
Education						
Boards of Education	.9951	.0019	.0229	.791	.840	.07
Chief School Officer	.9962	.0032	.0213	.731	.777	.14
Compulsory Attendance	.9960	.0075	.0216	1.019	1.000	.05
Degree, Elementary	.9931	.0130	.0268	1.014	.966	.07
Degree, High School	.9976	.0013	.0175	.895	.924	.15

* L = limit on the pool of adopters.

merit system, fair employment, and AFDC; negative intercepts for merit system and AB; a large intercept for AFDC; large standard errors for merit system and AFDC. More error seems to occur when attempting to predict the processes which have diffused completely. A more substantive interpretation is that error in prediction occurs for welfare laws; an explanation for this phenomenon will be offered in the section comparing issue areas.

The estimates of the parameter b in column 6 of Table 2 are also of interest; b can be interpreted as the probability that when "leaders" from two states meet, their interaction results in another adoption. Unfortunately, there is no criterion by which to evaluate the accuracy of

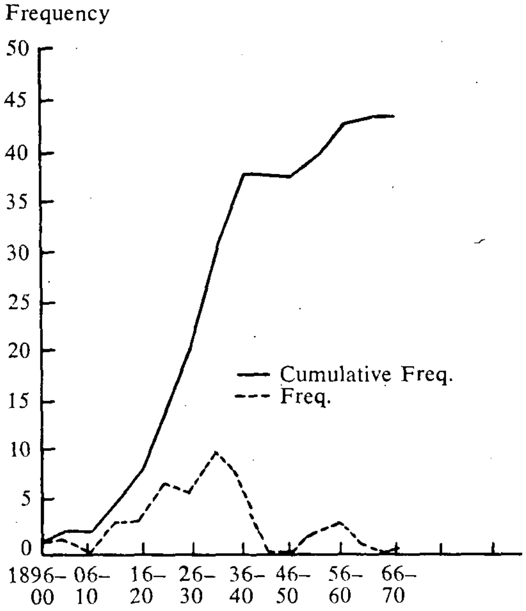

Figure 2. Adoption of degree requirement for teaching in high school by state, 1896–1966

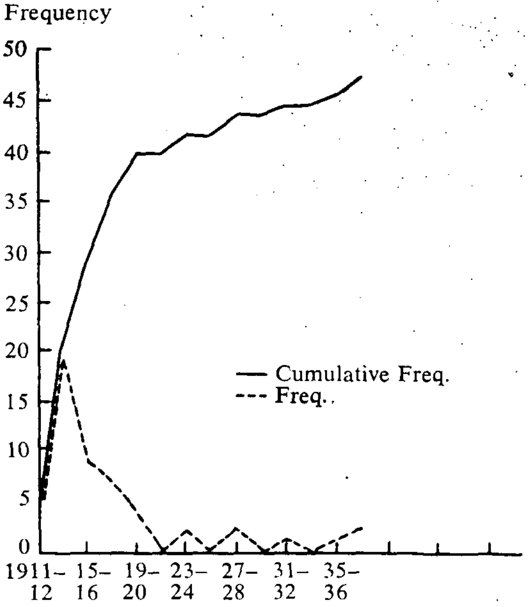

Figure 3. Adoption of aid to families with dependent children legislation by state, 1911–1937

these probabilities.[23] Interaction has the greatest impact in the diffusion of old-age assistance legislation. The fact that most of the b's are small is understandable, since for the graphs of most of these twelve laws a straight line with a "tail" at each end would fit. Therefore, a linear model was also evaluated, using the regression equation,

$$(7)\quad A_t = bA_{t-1} + c + e,$$

where the symbols are those defined earlier.

The R^2's of the quadratic model can be compared to the R's of the linear model, using the F-statistic as the test for significant difference.[24] As shown in Table 3, for 6 of the 12 models there is a significant increase in the proportion of variance explained when the interaction term is added. Consequently, in half of the cases studied, innovations seem to diffuse through interaction.

Table 3. Comparison of Quadratic and Linear Models for Twelve Innovations

Laws	Quadratic R^2	Linear R^2	F
Civil Rights			
Public Accommodations	.9756	.9750	.45
Fair Housing	.9841	.9831	1.56
Fair Employment	.9615	.9614	.04
Welfare			
Merit System	.9453	.9415	3.89*
Old Age Assistance	.9772	.9696	4.37*
Aid to the Blind	.9931	.9918	8.66*
AFDC	.9380	.9376	.18
Education			
Boards of Education	.9951	.9950	4.66*
Chief School Officer	.9962	.9958	12.33*
Compulsory Attendance	.9960	.9958	2.82
Degree, Elementary	.9931	.9928	1.63
Degree, High School	.9976	.9967	22.00*

* Significant at .05 level.

This simple interaction model holds up fairly well under evaluation on the basis of the criteria used above. The results generally confirm that some of these innovations diffuse as do others—through the interaction of users and nonusers. The next section compares the results by issue area. Certainly, one might want to refine the model, perhaps by adding a term for constant source diffusion and by relaxing the assumption of a completely intermixed population.[25]

Diffusion Patterns by Issue Area. Results from evaluating the diffusion interaction model can be put to a further use: comparing the three policy areas—education, welfare, and civil rights—with respect to diffusion from interaction. Several analyses of state expenditures, broken down by functional area, report that states vary in their level of support for each function, sometimes in response to the varied nature of the stimulus from federal grants-in-aid.[26]

Table 2 contained several points at which the issue areas differ. The average R^2 for civil rights is .9637; for welfare, .9634; for education, .9956. Innovations in education appear to occur with more regularity, possibly because the process took place over a longer period than in the other issue areas. Furthermore, within the area of education, there is little variation in R^2's among the various laws, while in the other two areas, there is marked variation in the amount of variance explained. Thus, one could say that education innovations diffuse in a regular and similar manner, for whatever reasons, whereas civil rights and welfare adoptions do not follow a single diffusion path unique to the subject matter of the law.

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[23] In general, the accuracy of b may depend upon the degree to which a social structure is completely intermixed. In this case, bias may be introduced by structural characteristics, such as regionalism, which reduce the number of relations *across* regional boundaries and increase the state contacts *within* regional communication networks. For an excellent discussion of communication networks at the state level see: Walker, "The Diffusion of Innovations Among the States," pp. 891–897.

[24] John Cohen, "Multiple Regression as a General Data-Analytic System," *Psychological Bulletin,* 70 (1968), 435.

[25] It should also be noted that several sources of error are possible in this model. First of all, the data are recorded as proportions based on 48 states; obviously, there were less than 48 states in existence for many years studied. Nevertheless, the model fits best for the longer processes (i.e., education) in which more states are absent; hence, this possible source of error did not have deleterious effect on the goodness of fit. Another kind of error could arise from using a lagged dependent variable on the right-hand side of the regression equation; the assumption that the stochastic or disturbance term is normally and independently distributed may be violated in such a situation. A common test statistic for the presence of serial correlation (i.e., the disturbance at t is highly correlated with the disturbance at t-1) is the Durbin-Watson d. When proper adjustments are made for using a lagged dependent variable, the d-test indicates no serial correlation. (See Carl F. Christ, *Econometric Models and Methods* [New York: John Wiley & Sons, 1966], p. 522). Another test for normality is to collect the residuals into a frequency distribution and graph them. (See Christ, pp. 526–530). If the residuals are approximately normal, the graph will be bell-shaped. When this test was applied to three of the laws, the residuals did not appear by inspection to be normally distributed. Thus, the results of the two tests are conflicting concerning the error terms.

[26] See Virginia Hickman Gray, "Theories of Party Leader Strategy and Public Policies in the American States." (Doctoral dissertation, Washington University, 1972).

It is interesting to note from Table 3 that the interaction term makes a difference for 3 of 5 education laws, 3 of 4 welfare laws, and no civil rights laws. This tendency fits in with the other findings, all generally pointing to the conclusion that the normal diffusion of civil rights laws is met by resistance from particularly immune states.

Although the simple diffusion model does not allow for it, one might also differentiate patterns of adoption according to the amount of diffusion from a constant source relative to diffusion from interaction. For instance, one might hypothesize that program adoptions tied to federal grants-in-aid will diverge from the pattern of normality exhibited by programs adopted independently by states. For this reason, wherever possible, laws were chosen for this study from areas relatively untouched by the federal government. In the welfare area, programs were selected which began independently, though the last few observations (those after 1935) are of programs falling under federal aegis.

Walker in his landmark article does not anticipate much difference:

> In a later work I will report the results of comparisons of the diffusion patterns of issues from different subject matter areas. Preliminary efforts at such comparisons, however, have not revealed significant variations. There does not seem to be much difference in the diffusion patterns of issues of different types.[27]

Walker makes up his composite innovation score from 88 programs which represent a mixture of federal grant-in-aid and independent state programs. He includes five state programs which are used in this study: compulsory school attendance, fair housing, teacher certification at both levels, and superintendence of public instruction. Their graphs are displayed in Figures 4–7 and in Figure 2. In the welfare area he uses program adoptions under the Social Security Act. The course of their adoption over time is displayed in Figure 8. There is a large initial enactment, and then adoptions drop off rapidly. Their diffusion pattern is radically different from that in any graph previously shown in this study. It seems, therefore, that diffusion patterns vary, even in Walker's own data, owing, apparently, to federal intervention.

The hypothesis that federal involvement is a source of variation in adoption patterns is also

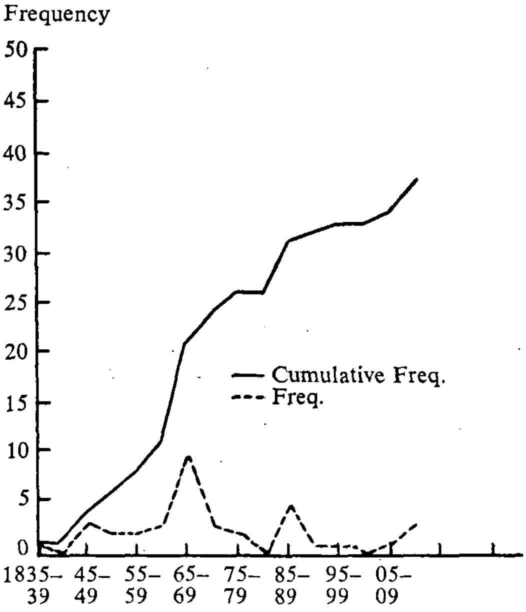

Figure 4. Adoption of chief state school officer legislation by state, 1835–1912

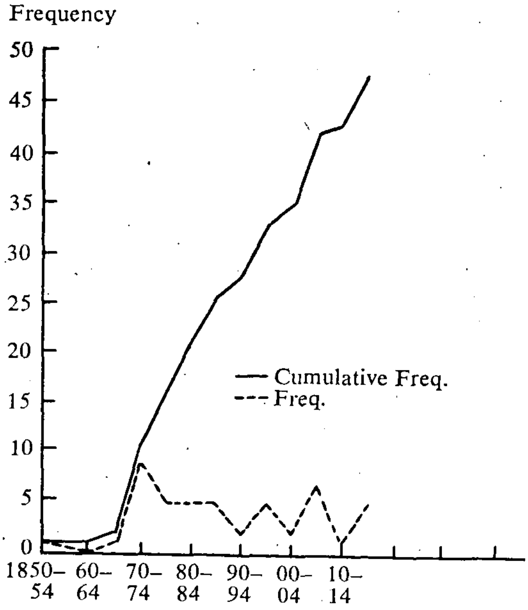

Figure 5. Adoption of compulsory school attendance legislation by state, 1850–1918

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[27] Walker, "Diffusion of Innovations Among the States," pp. 882, n. 9, or Jack L. Walker, "Innovation in State Politics," in *Politics in the American States*, 2nd ed.; ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Co., 1971), p. 591, n. 9.

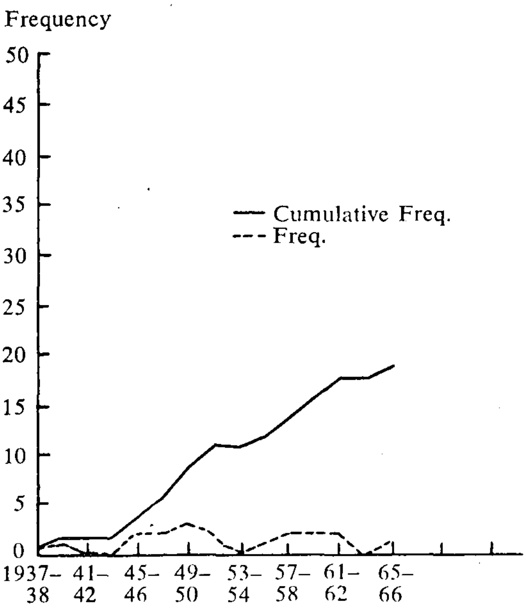

Figure 6. Adoption of fair housing legislation by state, 1937–1965

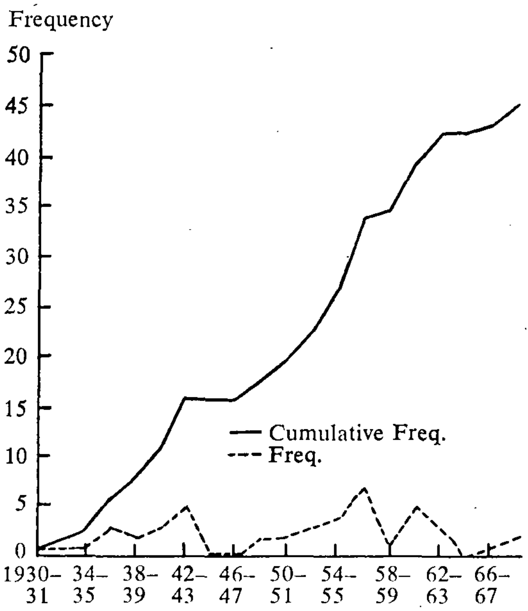

Figure 7. Adoption of degree requirement for teaching in elementary school by state, 1930–1969

borne out by the graph in Figure 9 for state merit systems covering welfare. Only nine states had such coverage before the Social Security Act was passed. After its passage 32 states decided to place their employees under a merit plan. Following the Social Security Board's successful fight to make merit plans a requirement, the remaining seven states followed suit. As a result, the pattern of spread for this innovation is somewhat different from that of other welfare policies. These results indicate the necessity for at least distinguishing between state and state-federal control in this dimension of policy and perhaps distinguishing among various types of federal aid to states.

Sources of Innovativeness

The second consideration of this study is the question: Why do some states adopt before others? Walker hypothesizes that demographic (socioeconomic) and political factors are among the more important preconditions for innovation just as they are for expenditures; hence, his prediction is that the wealthier and

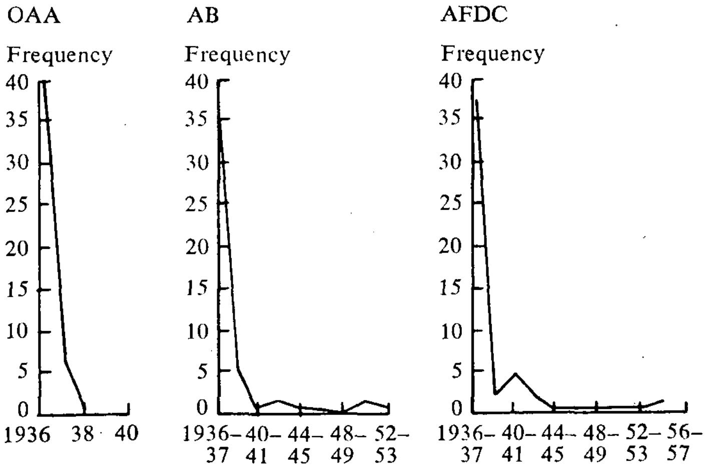

Figure 8. Adoption of Social Security programs by state, 1936–1955

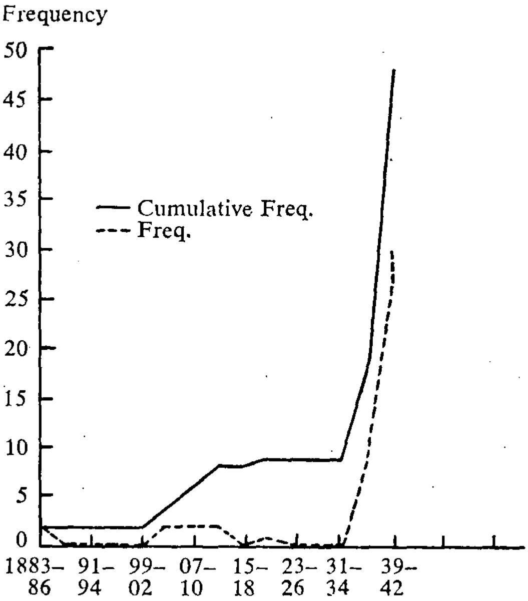

Figure 9. Adoption of welfare merit system legislation by state, 1883–1942

more competitive states ought to be more innovative. He found a strong positive correlation between average income and his innovation score in three time periods and little relationship between party competition and the composite innovation score for 95 years, when controlling for the demographic variable.[28]

The averaging of innovativeness, party competition, and income over nearly a hundred-year period may serve only to obscure whatever relationships exist between policy and the political system. This investigation of the reasons for innovation will be based on a less rigorous test of the above political and economic hypotheses. The question of interest is why a state adopts a law at a particular point in time. Part of the answer (to why states adopt at all) lies in user interaction, as demonstrated earlier in this article. User interaction, however, is not a full explanation because it cannot account for the first adoption. Political and economic explanations may be more relevant to the most and least innovative states (i.e., why the first is susceptible and why the last is most immune), while user interaction might better account for the order of the states falling in the middle range of innovativeness. Therefore, this part of the inquiry will focus on the political and economic differences between the first adopters and the rest of the states at the time of adoption. A similar kind of analysis could not be performed for the laggard states because for several laws, diffusion is not yet complete.

Analysis of First Adopters. One might predict that the most innovative states (defined as the first ten states to adopt any paricular law) would be above average in wealth and competitiveness in whatever year they adopted the law. The wealth measure used is per capita personal income which is available since 1922. The party competition measure is the governor's electoral margin in the most recent general election.[29] An appropriate test is to compare an early adopter's competition (or wealth) score in the year of adoption to the mean competition (or wealth) score for all 48 states in that particular year. The early adopter's score should be above the nationwide mean in each case.

There are 120 possible observations on which to make the test (12 laws times the top 10 states). Unfortunately, much of the pertinent data is missing from standard sources because the adoptions occurred very early for many of these laws. There are only sixteen observations for party competition. Of these, twelve (75 per cent) are in the predicted direction: competition is higher in innovative states. There are 40 observations for per capita personal income. Thirty-four (89 per cent) are in the predicted direction: innovative states are richer.

Thus, it appears that innovative states are both wealthier and more competitive than their sister states at the time of adoption of a particular law. This finding is consistent with the hypotheses derived from studies of state and local expenditures.[30] The small amount of data is not sufficient for testing the independence of the two variables. Also the available data were in the fields of education and civil rights only. The relationships discovered were more clear-cut for civil rights laws than for education laws. This finding, along with the fact that education laws diffused in a more regular pattern than civil rights, is evidence that education may be the least politicized of the three policy areas. The following section documents one case in which politics was particularly important.

The Case of Mothers' Aid Legislation. As noted earlier, the user interaction model failed to account for the unique diffusion pattern displayed by AFDC, or Mothers' Aid, as it was called at its inception. Also the distribution of adopters is unusual as was shown in Figure 3. For these reasons another explanation of the fact that eighteen states adopted such legislation in 1913, the second year of its diffusion, should be sought.

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[28] Walker, "Diffusion of Innovations Among American States," 884, 886.

[29] For more complete explanation of these measures and their data sources, see Gray, chap. 2.

[30] Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in the American States," *Journal of Politics,* 25 (May, 1963), 265–289.

The origin of the idea seems to have come from the "White House Conference on the Care of Dependent Children" called by President Theodore Roosevelt in 1909. The theme of that conference was that home care for dependent children is preferable to their being placed in institutions.[31] The innovation was opposed by social workers;[32] it was popular with legislators, however, because no increased taxes were anticipated.[33] Thus, the presidentially sponsored conference may have been indirectly responsible for the rapid dissemination of the information, though not for rapid adoption of the legislation.

Certainly the outstanding political feature of this era was the Progressive Movement, and it is reasonable to suppose that politicians of Progressive sympathies would support social welfare legislation. Dewitt, writing in 1915 on the Progressive Movement, said that the social phase of the movement was by far the most important at the state level and that Progressive reformers advocated mothers' pensions.[34] In fact, a history of Progressivism in Ohio gives credit for Ohio's mothers' pensions to James Cox, a Democratic governor with Progressive sympathies elected in 1912.[35]

In order to test for Progressive influence on the time of adoption, the states were divided into two groups: the eighteen states which adopted in 1913, and the 28 states which adopted later and for which data are available. For each group of states the mean percentage of Progressive party strength was computed, based on the elections for the governorship and the legislature immediately prior to 1913. This figure should underestimate the strength of Progressivism since politicians running on the regular party ticket also may have had Progressive sympathies.

The means for each group are displayed in Table 4. States which adopted mothers' aid legislation in 1913 averaged much greater Progressive strength than did states adopting later. Apparently, this is one case where politics played an important part in the timing of an innovation. In the following section the investigation focuses on patterns of innovation.

Patterns of Innovation. Throughout this study each innovation has been studied separately, in contrast to the method used by Walker. The previous sections have demonstrated the utility of disaggregation. Indeed, one might question the fundamental assumption of a "composite innovation score"—namely, that "innovativeness" exists as a single factor among states. Operationally, the question becomes "Do the states which are early to adopt one law also adopt other laws first as well?"

Table 5 shows how states rank in each of the three issue areas, and how they rank overall. The states were ranked from 1 (first) to 48 (last) on time of adoption of each law. Then a state average was computed for each issue area and overall. The overall ranking is similar to Walker's, partly because five of the laws included here were the same as his. There are some notable differences, however; Pennsylvania is 20th on this scale and 7th on Walker's; Nevada is 21st on this scale and 47th on Walker's; Louisiana ranks 19 on his scale and 36.5 here.

The interesting information that is concealed by a simple average ranking is the range for any one state. New York and California are the most innovative states; yet each has failed to adopt one of the laws in question. South Dakota is the most laggard state; nevertheless, it was among the first ten for one law. Thus, any study of policy innovation using averages

Table 4. Extent of Progressive Influence in States, According to Time of Adoption of Mothers' Aid Legislation

	Mean % Progressive Party Strength in Last Election Before 1913	
	Timing of Adoption	
Electoral Contest	1913 Adopters	Later Adopters
Governor	15.2%	6.5%
Lower House	7.0	.5
Upper House	6.0	1.2

Source: The World Almanac and Encyclopedia, 1914 (New York: Press Publishing Company, 1913), pp. 729–778.

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[31] Fred S. Hall, ed., *Social Work Yearbook, 1929* (New York: Russell Sage Foundation, 1930), pp. 131, 274.

[32] James Leiby, *Charity and Correction in New Jersey* (New Brunswick: Rutgers University Press, 1967), p. 94.

[33] Ada J. Davis, "The Evolution of the Institution of Mothers' Pensions in the United States," *American Journal of Sociology*, 35 (January, 1930), 582.

[34] Benjamin Parke DeWitt, *The Progressive Movement* (New York: Macmillan Co., 1915), p. 253.

[35] Hoyt Landon Warner, *Progressivism in Ohio, 1897–1917* (Columbus: Ohio State University Press, 1964), p. 404.

across issue areas may conceal phenomena of great potential interest.

Table 5. Ranks of the States According to Order of Adoption of Laws, Averaged by Issue Area

State	Overall Average Rank	Average Education Rank	Average Welfare Rank	Average Civil Rights Rank
California	13.0	6.1	11.5	26.5
New York	13.0	19.2	12.5	3.3
Massachusetts	14.8	25.0	8.7	6.0
New Jersey	16.0	24.9	13.3	4.8
Wisconsin	16.2	27.7	5.0	12.3
Connecticut	16.2	16.4	24.3	5.1
Washington	16.4	18.0	17.7	11.8
Colorado	16.8	26.3	9.3	11.1
Michigan	16.8	16.0	20.3	13.6
Illinois	17.2	25.0	10.1	13.6
Ohio	17.2	25.7	10.3	15.6
Idaho	19.0	23.6	12.1	20.5
Oregon	19.0	20.9	23.7	9.6
New Hampshire	19.9	22.4	16.3	20.5
Maryland	20.0	15.5	15.7	33.5
Indiana	20.4	16.1	33.5	10.1
Kansas	21.4	14.2	29.5	22.6
Utah	21.5	17.6	22.2	40.3
Minnesota	22.1	39.1	11.3	8.1
Pennsylvania	22.9	27.1	25.5	12.6
Nevada	23.5	24.0	10.5	40.3
Wyoming	25.3	27.6	21.7	39.8
Montana	25.5	29.7	25.7	18.1
Rhode Island	25.5	28.1	35.7	7.8
Kentucky	25.7	21.3	23.8	35.6
Vermont	25.9	18.1	29.2	37.8
Missouri	26.6	21.7	28.3	32.6
Arizona	27.2	22.8	27.6	34.1
Iowa	27.5	30.0	16.8	37.8
Delaware	27.8	25.6	28.1	31.1
Nebraska	28.2	35.7	18.8	28.3
Maine	29.6	34.4	18.8	36.0
New Mexico	29.7	25.2	38.3	26.0
North Carolina	30.2	17.5	42.7	48.0
West Virginia	30.9	25.4	25.1	48.0
Louisiana	31.7	21.1	32.8	48.0
Texas	31.7	23.0	41.1	48.0
Tennessee	31.9	23.2	30.7	48.0
South Carolina	34.3	24.4	36.5	48.0
Arkansas	35.0	30.9	30.3	48.0
Florida	35.0	28.4	33.5	48.0
North Dakota	35.4	38.1	28.7	39.8
Virginia	36.0	25.5	40.2	48.0
Oklahoma	37.1	36.2	30.1	48.0
Alabama	37.1	28.2	35.3	48.0
Mississippi	37.4	32.0	36.3	48.0
Georgia	37.7	31.0	38.5	48.0
South Dakota	38.4	37.9	32.0	48.0

Spearman rank-order correlations can be utilized to answer more directly the question of the stability of "innovativeness." If there exists a stable factor of "innovativeness," then states ranking high (or low) on a scale of time of adopting any one law ought to rank high (or low) on a time of adoption scale for another law. Certainly, within an issue area this principle ought to hold; i.e., the correlation between one education ranking scale and another education ranking scale should be positive. States were ranked from 1 (first) to 48 (last) on each of the twelve laws according to time of adoption.

Table 6 presents the correlation of the rank on one innovation scale with the rank on every other innovation scale. In general, the strength of the correlations is very low. Only eleven of 66 (1/6) are .50 or above; sixteen (about 25 per cent) are negative. About half of the stronger correlations are between laws which diffused during roughly the same time period. One might infer that some states are innovative at one point in time, but they are not necessarily innovative at another point in time; hence, "innovativeness" should not be aggregated over long time periods.

If "innovativeness" does not occur as a general timeless phenomenon, perhaps it is issue-specific, i.e., the same states might be innovative in all phases of education but not innovative on other issues. Table 7 shows that only for the issue of civil rights does the phenomenon of "innovativeness" truly appear to exist: the average of states' rank intercorrelations on civil rights laws is .67. States which are leaders (or laggards) in adopting one civil rights law are leaders (or laggards) on other civil rights laws. This finding, juxtaposed with those earlier concerning the civil rights distribution's stronger association with wealth and competition, is further evidence that civil rights laws are more politicized than are the laws in the other two policy areas.

Contrast this with the situation in the education field where not only is the adoption of education laws unrelated to adoption of laws in welfare and civil rights, but adoption of one education law is only slightly related to adoption of another education law; the average rank correlation among education laws is .17. Probably the length of time is one explanation, e.g., adoptions of one education law extend over the period 1784–1969. Also the five education laws are probably more diverse in substance than the set of three civil rights laws which evoke more powerful public reactions.

Another reason for the high intercorrelation among civil rights laws is that they diffused later in a shorter time period; many states have not yet adopted them. Technically, this means that there are a larger number of ties on the civil rights rankings; therefore, a correction was made for ties which has the effect generally of reducing the strength of the correlation, though the magnitude of the reduction was slight for these three laws.[36] At any rate, the civil rights rankings reported here are corrected for ties

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[36] Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences* (New York: McGraw-Hill Book Co., 1956), p. 210.

Table 6. Spearman Rank-Order Correlations (rho) For 12 Innovation Rankings

	Education				Welfare				Civil Rights		
Laws	Chief School Officer	Compulsory Attendance	Degree for Elem. School	Degree for High School	OA	AB	AFDC	Merit System	Public Accommodations	FEPC	Fair Housing
State School Boards	.07	.06	.27	.18	−.06	.12	−.22	−.04	−.09	.20	.08
Chief School Officer		−.29	−.16	−.05	.01	.03	−.03	−.16	−.28	−.13	.00
Compulsory Attendence			−.08	.75	.43	.36	.55	.38	.60	.50	.53
Degree for Elem. School				.42	−.11	−.17	−.11	.12	−.08	.30	.27
Degree for High School					.02	.09	.01	.20	.07	.21	.10
OA						.38	.52	.37	.36	.38	.48
AB							.50	.37	.46	.39	.37
AFDC								.37	.37	.46	.55
Merit System									.45	.49	.48
Public Accommodations										.65	.65
FEPC											.71

and are still much different from all other uncorrected rankings.

Welfare occupies an intermediate position in respect to intercorrelation. There is a moderate amount of underlying stability within the welfare dimension but slightly more relationship between the welfare and civil rights areas than within welfare itself.

Table 7. Spearman Rank-Order Correlations for Date of Adoption, Averaged by Issue Area

	Education	Welfare	Civil Rights
Education	.17	.07	.15
Welfare		.41	.44
Civil Rights			.67

Summary

Innovations (adoption of laws by states) were studied in three "have-not" oriented policy areas: education, welfare, and civil rights. A model of the diffusion process based on user interaction was constructed; it performed fairly well under evaluation by several common criteria. Graphical analysis showed, however, that diffusion patterns do differ by issue area and by degree of federal involvement.

Political and economic explanations proved to be useful in determining which states are the first to adopt laws. A brief case history of the adoption of Mothers' Aid legislation pointed up the strong effect of Progressive sympathies on early adopters. Finally, it was shown that "innovativeness" is not a pervasive factor; rather, it is issue-and time-specific at best.

Comment: Problems in Research on the Diffusion of Policy Innovations*

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Diffusion theory has been an important tool for analysis by geographers, sociologists and economists, but the diffusion of diffusion theory among political scientists, so far, definitely does not approximate an S-shaped curve! In fact, as far as I know, Professor Gray's is only the second article in *The Review* that employs the word "diffusion" in its title.[1]

The obstacles to the spread of diffusion theory among political scientists do not exist in the essential nature of political phenomena. The American federal system can usefully be conceived as an elaborate diffusion process, as Gray's article demonstrates, and many other cases where distinctive approaches to policy have evolved within some part of the governmental system might better be understood if seen as examples of diffusion. The reasons for political scientists' neglect of diffusion theory, of course, lie not in the peculiarities of their subject matter, but in the prevailing division of labor among scholarly disciplines in American universities. The various branches of learning stake out their special preserves and claim exclusive jurisdiction over certain variables, research problems, and explanatory theories—diffusion processes have been centrally important for anthropologists but have rarely been employed by students of politics.

There is no good intellectual justification for restricting the use of any explanatory scheme to a limited class of social problems. All the social sciences need as much theoretical help as they can get. Certainly, Gray's article, with its references to material from several disciplines, is a model of the ecumenical spirit and a good example of the progress that can be made through judicious borrowing from other scholarly traditions.

Professor Gray set out in her article to "extend in a more rigorous fashion the investigation of innovation by states"[2] that I began four years ago. It would be unduly coy of me not to express satisfaction at this evidence that another political scientist found my work stimulating enough to use it as a guide for further investigation. The mathematical model that is the centerpiece of this article should be useful for analyzing data in several other settings and should stimulate further interest in diffusion theory within our discipline. It is important to see, however, that Gray has not only employed different techniques of analysis than I did, but has also studied a somewhat different aspect of the diffusion process, and has structured the problem in a different way. These crucial variations in our approaches seem to have led to some misunderstanding between us about the use of terms, and they may account for some of the apparent differences in our findings.

Our two articles attack the same general problem, using similar data, but with contrasting assumptions and ultimate aims. Our findings differ in some important ways, but because this is still a largely unexplored area, the most significant aspect of the two presentations is the kind of guidance they offer for future research. In this comment, besides defending my own approach which I believe takes us in more fruitful directions, I shall attempt to provide insights into the principal conceptual and methodological problems that face anyone wishing to investigate the diffusion of public policies among the American states, or among nations or other governmental units. The comment begins with a brief résumé of my own article, and proceeds with an analysis of the way Gray and I have answered some of the more serious questions of measurement and inference that arise in this field.

Curves Versus Spreading Ink Blots

The central problem I dealt with involved

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* Thanks are due to James Alt of the University of Essex, Sidney Winter, my colleague at the Institute of Public Policy Studies at the University of Michigan, and my wife, Linda G. Walker, who read an earlier draft of this comment. Their criticisms and suggestions saved me from many mistakes of inference and expression, but what finally emerged is entirely my responsibility.

[1] See Kenneth Janda, ed., *Cumulative Index to the American Political Science Review* (Evanston, Ill.: Northwestern University Press, 1964).

[2] Virginia Gray, "Innovation in the States: A Diffusion Study," *American Political Science Review*, 67 (December, 1973), pp. 1174–1185. The article to which she refers is: Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 880–899. A revised version of this article appeared in: Herbert Jacob and Kenneth N. Vines, eds., *Politics in the American States*, 2nd edition (Boston: Little, Brown and Co., 1971), pp. 354–387.

the relative speed with which states adopted new ideas. I was interested in whether some states characteristically adopted new programs earlier than other states and if so, how this phenomenon might be explained. I began my research by developing a measure of the average speed of adoption for all the states and then tried to discover its economic, social and political correlates. Gray questions the validity of this index of innovativeness because she doubts that states remain peculiarly innovative over a long period of time or that a quality called "innovativeness" could ever be isolated and independently described. I disagree with her over this question, but more about that below. The important point I wish to make at the outset is that I was primarily concerned with the propensities of states to adopt new programs and attempted to create an empirical index of this characteristic, averaged over many possible adoptions. I was not concerned, as is Gray, with the cumulative frequencies of adoptions over time or whether the pattern of adoptions approximated an "S"-shaped curve.

I did refer in my article to "patterns" of adoption, but did not use the term to refer to cumulative frequencies. I advanced the hypothesis that a typical diffusion pattern existed in which one of the pioneering states adopted first, followed by other pioneers, and finally by other states which tended to take cues from their regional pioneers. This diffusion process forms an essentially geographical pattern, and can be visualized as a succession of spreading ink-blots on a map created by the initial adoptions of new policies by states playing in a national "league" of cue-taking and information exchange, followed by other states whose standards of comparison and measures of aspiration are more parochial and who typically adopt new policies only after others within their regional "league" have done so.

In view of these differences in our use of the phrase "pattern of adoption" I believe Gray is unfair in charging that I did not see differences between the patterns of adoption of innovations that arose from state initiative and those that were launched at the federal level or were subject to federal intervention at some point in the course of their diffusion.[3] As I gain experience in research and the distractions of university life I can see that it is a mistake ever to promise further work on a problem, even if one honestly expects to do it, but I did spend some time with the question of federal intervention and found, as I reported, little difference in the rank ordering of states on such issues, or in the essentially geographical patterns I was looking for.

Gray's analysis of the shape of diffusion curves clearly leads to the presumption that federal intervention makes a difference in the temporal frequencies of adoption, generally by accelerating the process and encouraging the spread of innovations to all the states. This is an important finding that might eventually allow the classification of issues according to differences in their diffusion patterns. Of course, programs that diffuse to the great majority of states within only three or four years, such as Aid to the Blind or Old Age Assistance, even if they constitute an analytically separate class of issues within some broader theory of diffusion, nevertheless are difficult to study because of their speed of diffusion. Before this aspect of Gray's inquiry can be exploited, a wider selection of programs affected by federal intervention must be analyzed, but this line of research appears to offer many interesting possibilities. It should be understood, however, that Gray and I were interested in different kinds of diffusion patterns, each important in their own right. Even though our data were similar, our discoveries were different because we were asking different questions, as well as using different methods.

Regional Barriers to Interaction

This apparent misunderstanding between us over the meaning of the phrase "diffusion patterns" helps to reveal the differences in our projects and to show how much explanatory power Gray has had to give away in order to achieve the kind of rigor that characterizes her presentation. One central assumption upon which her diffusion model is founded is that all states may be treated alike for the purposes of analysis, or to use her terminology, she assumes that she is working with "a completely intermixed population."[4] My research specifically was designed to show that the American states are *not* a completely intermixed population, but rather are organized into a complex system of pioneers, regional leaders and laggards. I identified some important developments that were creating pressures for national uniformity, such as the growing cosmopolitan communities of public officials and policy experts, and made some rather oblique efforts to measure these trends, but my conclusion was that regional differences were remarkably persistent and unlikely soon to disappear. Gray certainly recognizes that regional biases in the flow of communications among adopters may make it unreasonable to regard the states as a completely intermixed population and refers several times to the possibility of creating more elaborate models of diffusion by relaxing this fundamental assumption,[5] but presumably this would be a much more difficult undertaking that would not have the economy or simplicity of her present model. These remarks are not meant to deny the obvious benefits of creating formal models, but merely to underline the limitations of this one as a description of reality.

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[3] Gray, p. 1185.

[4] Gray, p. 1176, 1179.

This questionable basic assumption hinders Gray in explaining the diffusion of civil rights bills, for which she reports that the "normal" diffusion process, based on the interaction among decision makers, "is met by resistance from particularly immune states."[6] These fourteen resistant states, of course, with one exception (South Dakota), are contiguous and clustered in the South.[7] It might be an interesting test of Gray's model if she removed the thirteen southern states, or perhaps only the eleven states of the Confederacy usually designated as southern in such studies, and ran the model for the remainder, among which the assumption of complete population intermixture might be more legitimate. If this exercise were performed on the states outside the South, the interaction term in her diffusion model might begin to assume the importance it does for most cases in education and welfare.

Regional peculiarities that produce "interaction effects"[8] in her data also call into question Gray's assertion that civil rights legislation is more likely to be caught up in partisan disputes than are topics such as teacher certification or AFDC. This inference grows from several findings, including the fact that rank orderings of the states according to the speed of adoption of civil rights bills have stronger associations with measures of relative wealth or political competition than rank orderings based on legislation in the other two categories. These high correlations, however, may result only from the tight clustering of southern states at the bottom of all these scales, a fact that may have arisen merely by coincidence. Findings of this kind may be only a reflection of the great differences between the South and the rest of the country and may not be an indication of functional relationships among these variables within all the states. If states in the South and those in the rest of the country were treated as independent groups the relationships between the speed of civil rights adoption and wealth or political competition may disappear within each group. In research of this kind, where such distinctive regional clustering appears, correlations should be run with the distinctive region removed before generalizations or further broad inferences are made about functional relationships or causative factors.[9] A more elaborate model that takes account of regional barriers to interaction among potential adopters would still be needed, even if such tests were made, but these extra pieces of analysis might prevent Gray from drawing misleading conclusions from findings that may only be statistical artifacts.

Differences Between Issues and Adopters

The differences in the fundamental assumptions Gray and I make concerning the nature of the system we are investigating have helped to determine our choices of research strategies as well as the course of our analyses. Gray, for example, argues that each policy innovation should be studied separately so that the peculiarities in each diffusion pattern can be discovered and accounted for. She doubts the usefulness or validity of my efforts to create a composite index of innovation because she does not believe that any quality called "innovativeness" can be attached to any single state government for any significantly long period of time.[10] Her assumption of the interchangeability of states, of course, is consistent with the careful analysis of individual issues. By testing each distribution of cumulative frequencies of adoption against the prediction that it will approximate normality Gray has an unchanging standard for comparison. Using this method her attention usually is directed to the peculiarities of the issues under examination that cause them to diffuse in an "abnormal" way—whether they are presented for adoption with accompanying federal grants-in-aid, for example, or stimulate unusual amounts of partisan controversy. This research strategy is akin to those used in studies of the diffusion of technological artifacts to farmers or small businesses where assumptions about the complete intermixture of the populations are appropriate, or at least not so risky as in the case of a vast, continentwide governmental system.[11] Gray's method has yielded several important insights, as the case study approach often does, and my own work probably suffered from inadequate sensitivity to the importance of the characteristics of policy innovations as determinants of the course of their diffusion. Nevertheless, as long as Gray takes up issues one at a time and concentrates her attention on the nature of the objects being diffused she is unlikely to discover systematic or recurring differences in the responses of individual state governments to the prospects of change—the problem that was my special concern.

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[5] Gray, pp. 1176, 1179, and footnotes number 21, 23 and 25.

[6] Gray, p. 1180.

[7] These data are provided by Gray in Table 5, p. 1184.

[8] The phrase "interaction effects" is being used as it was in: Hugh Donald Forbes and Edward R. Tufte, "A Note of Caution in Causal Modelling," *American Political Science Review*, 62 (December, 1968), 1261–1262. For similar commentary see the discussions of the "universal fallacy" and the "contextual fallacy" in Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965), pp. 101–106.

[9] For a more lengthy presentation of this view, see: Dennis Riley and Jack L. Walker, "Communication," *American Political Science Review*, 63 (September, 1969), 900–903. For an interesting discussion of analytical problems created by regionalism, see: John L. Sullivan, "Political Correlates of Social, Economic, and Religious Diversity in the American States," *The Journal of Politics* (February, 1973), pp. 70–84.

[10] Gray, pp. 1183–1185.

In my own research, the composite innovation score played the same role that Gray's assumptions about normal distributions play in hers. That is, it provided a framework for analysis and a basis for comparison. My efforts were directed at explaining why some states, on the average, adopted new ideas more rapidly than others—why New York tended to adopt most new policies before Mississippi—and in this inquiry I drew most often on insights from theories that emphasize the importance of structural and procedural differences among organizations or business firms in the way they process information, search for new ideas, or measure their achievements.[12] My attention was focused on the agencies adopting the innovations and on those stable aspects of their environment—especially regionalism and the growing professionalization of public bureaucracies—that seemed to be shaping their behavior. Gray touches on most of these factors in her presentation, but her approach leads her to focus most of her attention on the traits of the innovations being diffused, a mode of analysis that is unlikely to uncover fundamental differences among the adopters. My concern was primarily with the decision rules being employed by those to whom proposals for change were being presented, and not so much with the importance of differences among the policy innovations themselves.

No researcher in this area will ever be presented with a simple choice between studying the nature of the innovations or the behavior of the adopters. Policy innovation and diffusion are much too complicated to allow anyone to ignore one side of the process altogether, although, as I have tried to show, most research designs place greater emphasis on one of the two aspects of the problem. This central analytical distinction is important, however, since it has significant consequences both for research design and for the formulation of generalizations and new hypotheses. Since limited resources usually require that attention be restricted to a small number of problems, most researchers must choose one emphasis over another, but decisions about these problems of research design are more than mere matters of taste. Even after reading Gray's paper, I still believe that efforts to develop a theory of organizational innovativeness and to create a better description of the specialized intergovernmental communication links within the American federal system offer greater potential theoretical payoffs. It is very likely that "innovativeness" is an impermanent condition, and that periods of pioneering innovation by a state government often are followed by periods of consolidation, or even stagnation, but this presents us with the challenging task of explaining these cycles of change. Even though Mississippi was the least innovative state on my composite index, it was the first to adopt the sales tax and recently has pioneered in allowing conjugal overnight visits for prisoners in its jails and penitentiaries.[13] This may indicate, as Gray's analysis suggests, that within certain classes of issues, innovations may originate on the periphery rather than in one of the system's cosmopolitan centers.[14]

We probably can never hope to account for more than a modest amount of the variance, however, in so immense and elaborate a system. The presence of a single aide on a legislative staff who is enthusiastic about a new program, or the chance reading of an article by a political leader can cause states to adopt new programs more rapidly than might normally be expected. We are studying an immensely complicated social system in which random occurrences and chance factors are prominent ingredients, but regardless of these difficulties I am not prepared as yet to give up my emphasis on organizational factors as keys to understanding, nor am I able to accept Gray's alternative notion that New York can be regarded as interchangeable with Mississippi or Arkansas, or that California can be placed, for purposes of analysis, in the same class as Wyoming, Vermont, or Rhode Island.

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[11] For a review of diffusion research that includes an extensive bibliograpry of more than 1,500 items see: Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations: A Cross-Cultural Approach* (New York: Free Press, 1971).

[12] The body of theory upon which I drew is summarized best in Donald W. Taylor, "Decision Making and Problem Solving" in *Handbook of Organizations*, ed. James G. March (Chicago: Rand McNally, 1965), pp. 48–86. Also see: Julia Feldman and Herschel E. Kanter, "Organizational Decision Making," *Ibid.*, pp. 614–649; W. Richard Scott, "Theory of Organizations" in *Handbook of Modern Sociology*, ed. Robert E. L. Faris (Chicago: Rand McNally, 1964), pp. 485–529; and Lawrence B. Mohr, "Determinants of Innovation in Organizations," *American Political Science Review*, 63 (January, 1969), 111–126.

[13] For a discussion of the genesis of the latter program and reactions to it in other parts of the country, see: Columbus B. Hopper, *Sex in Prison: The Mississippi Experiment with Conjugal Visiting* (Baton Rouge: The Louisiana State University Press, 1971).

[14] For an insightful article that illustrates this point in reference to innovations in public health, see: Marshall H. Becker, "Sociometric Location and Innovativeness: Reformulation and Extension of the Diffusion Model," *American Sociological Review* (April, 1970), pp. 267–282.

Innovation Scores and Sampling

Gray's demonstration that the twelve issues she dealt with were not strongly intercorrelated is not sufficient grounds for abandoning efforts at measuring the factor of innovativeness in state governments. She chose programs that were relatively free of federal influence and were objects of the struggle between haves and have-nots, because she wanted to evaluate the importance of political, rather than economic factors, that exist within the states rather than at the national level, as determinants of diffusion. Also, the issues she studied came from only three issue areas, compared with twelve in my study. Yet even though different criteria of selection were used in our two studies, the composite score Gray produced still roughly approximates the one I presented. Gray is certainly justified in claiming that a composite score may cover up interesting phenomena, and her article gives evidence of the insights that can be gained from disaggregation. Some merging of our techniques probably will be necessary to produce a full picture of this complicated diffusion process, but where the composite innovation score is concerned, my hunch is that most new scores based on a relatively broad selection of issues using almost any issue typology as guidance would very closely resemble the one that emerged from my data.

Admittedly, this is little more than an assertion of faith, but it cannot be given a conclusive empirical test until a method can be devised for selecting a representative sample of bills from whatever issue areas or policy arenas one choses to study. Neither Gray nor I have dealt adequately in our research with the problem of sampling. Hundreds of thousands of programs have diffused through the federal system during the past fifty years alone and we have no satisfactory way of cataloguing or listing them as a basis for sampling. Although each of us has constructed criteria for issue selection that seem reasonable, we have no guarantee that the issues we are studying are not highly unusual and somehow unrepresentative of their class. This problem is not serious enough, in my opinion, to halt inquiries into diffusion, but until more reliable sampling techniques are devised we must use caution in evaluating descriptive statements of any kind arising from anyone's research.

Reasons for Studying Organizational Innovativeness

Naturally, it would be possible to design a study that concentrated on the traits of the issues being diffused without also making the assumption that the American states can be treated as a completely intermixed population. I believe this assumption is a serious weakness in Gray's approach, as should be plain by now, but I do not want to make too much of it. There are other serious obstacles to building theories of diffusion that employ traits of the innovations as factors determining their diffusion patterns, and principal among these is the fact that the proposals themselves often are constantly being modified during the course of the diffusion process.[15] Can we safely make the assumption that the legislation providing Aid to Families with Dependent Children in the 1930s or 1960s is actually just another name for the Mother's Aid Legislation of the Progressive era? Is it not likely that these proposals have been so thoroughly modified that they are actually different bills and should be treated separately rather than as integral parts of a continuous process of diffusion? Without doing the necessary investigation, I cannot answer these questions, or similar ones that arise about the diffusion of state boards of education that took place over a period of 165 years. It will not be easy, however, to construct an objective method of deciding when an alteration in the details of a program is only a modification of the original or a change of such magnitude that it constitutes an innovation in itself worthy of independent analysis. This problem plagues all diffusion research that deals with phenomena as complex as social programs, and in principle it is not insurmountable, but until it is dealt with, the researcher who concentrates on studying the shape of diffusion curves cannot be sure whether the process being observed is a discrete event or whether it represents the aggregation of two or more independent diffusion processes.

Beyond these serious conceptual problems facing alternative approaches there also are positive reasons for innovation researchers to concentrate on such questions as the information sources utilized by decision makers, the prevailing decision rules being used by potential adopters, and the determinants of organizational goals or aspirations. It seems more likely that work on this cluster of problems will help us explain the first adoption of new programs by pioneering states before they begin to diffuse throughout the federal system. There have been few systematic attempts by students of diffusion to deal with this important aspect of the problem, just as political scientists have made little effort to discover the origins of public policies. With the notable exception of the dispute over "nonissues"[16] and some mainly speculative discussion of political culture,[17] political scientists generally have taken the issues that appear on the political agenda as given and have defined their task as the study of conflict resolution and the clash of vested interests, mainly in the electoral and legislative systems, that result from efforts to implement these proposals. The question of how issues evolve and present themselves for adoption has been given little attention.[18] One cannot study everything at once, unfortunately, and few of the problems of diffusion have been laid to rest, but if our theories of the way policies spread through the federal system are built from studies of decision making, cue taking, and the flow of information between organizations, they are more likely than explanations built from an inspection of diffusion curves to merge with theories of how problems are identified and new proposals created in the first place. The processes of innovation and diffusion go together, and so should theories that are meant to explain them.

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[15] For a more extensive discussion of this problem see: Kenneth E. Warner, "The Need for Some Innovative Concepts of Innovation: An Agenda for Research" (Working Paper Prepared for Faculty Seminar on Innovation and Social Change, Institute of Public Policy Studies, University of Michigan, 1972).

A Plea for More Research

Before setting out to investigate the diffusion of public policies among the American states, one must make several important decisions about the strategy and tactics of research. In this comment I have touched on some of the most important problems that arise at the earliest stages of research design, such as what fundamental assumptions about the nature of the federal system can legitimately be made, what kinds of findings can be taken as acceptable confirmation of central hypotheses, upon what bases issues should be selected or samples constructed, whether to focus mainly on the policies being diffused or to pay more attention to the adopting units—or how to merge these two concerns—and finally, what further theoretical problems or empirical questions can be attacked once the study being planned has been completed. All these questions would have to be dealt with, although in slightly different form, if one were to study the international diffusion of innovations[19] or the spread of new policies among cities or counties.

I hope this brief commentary on Gray's article provides realistic illustrations of how these questions affect research on diffusion and how the two of us differ in our approach to the subject. Although both approaches offer promise of opening up a previously uncharted region for more intensive scholarly exploration, our techniques differ and so do our answers to some of the fundamental conceptual problems in the field. All scholars are inflicted with the imperialistic notion that their field of interest deserves much greater recognition and attention from their colleagues. It is an academic commonplace to call for more research on almost any problem. In the case of the innovation and diffusion of public policies, however, more research, recognition, and interest actually are needed, so I do not feel reluctant to make such a plea. Neither Gray nor I have exhausted the possibilities for significant investigations in this area. It is my hope that Gray's article and this dialogue between us over the problems and challenges of research on diffusion and innovation will attract many more political scientists into this important field.

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[16] This literature is reviewed in Raymond E. Wolfinger, "Nondecisions and the Study of Local Politics," Frederick W. Frey, "Comment: On Issues and Nonissues in the Study of Power"; and Raymond E. Wolfinger, "Rejoinder to Frey's Comment," *American Political Science Review*, 65 (December, 1971), 1063–1104.

[17] I refer to: Daniel J. Elazar, *American Federalism: A View From the States* (New York: Crowell, 1966); and Edward C. Banfield and James Q. Wilson, *City Politics* (Boston: Harvard University Press, 1963). Also see: Raymond E. Wolfinger and Fred I. Greenstein, "Comparing Political Regions: The Case of California," *American Political Science Review*, 63 (March, 1969), 74–85.

[18] Two recent studies that include interesting efforts to explain the origins of policy are: Matthew A. Crenson, *The Unpolitics of Air Pollution* (Baltimore: Johns Hopkins University Press, 1971); and Mark Nadel, *The Politics of Consumer Protection* (Indianapolis: Bobbs-Merrill, 1972).

[19] For a study of the international diffusion of social security programs see: David Collier and Richard E. Messick, "Functional Prerequisites *Versus* Diffusion: Testing Alternative Explanations of Social Security Adoption," (Paper Delivered at the 1973 Annual Meeting of the Midwest Political Science Association, Chicago, Illinois).

Rejoinder to "Comment" by Jack L. Walker

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I enter the final round of this dialogue with the same scholarly intent Professor Walker has so ably expressed—to wrestle with the problems and challenges of research on diffusion of innovations in the hope of attracting attention to this relatively unexplored realm of policy research.[1] Perhaps we should be worrying about a more practical problem of diffusion: in Rogers and Shoemaker's review of the literature, including a bibliography of 1500 studies of innovation, political science is not recognized as having a tradition of research in this field.[2] Yet, both of our studies refer to this vast body of research in the other social sciences; mine in particular relies heavily upon previous theoretical work, resulting in some differences in the approaches of Walker and myself and thus sometimes in our different conclusions. Professor Walker has done an excellent job of summarizing those differences so I will confine myself to discussing first a few technical points on which we remain in disagreement; second, a more fundamental difference we apparently have on the usefulness of models in social science; third, the direction of future research.

Technical Points of Disagreement

As Professor Walker makes clear, his "patterns of adoption" refer to geographical patterns while mine refer to the tendency of the distribution of adoptions over time to approach a normal curve. His footnote in the original article asserting that there is little difference in the diffusion patterns of issues of different types appears in Section I, "Definitions and Distinctions," following an introduction where a long footnote discusses the theory of diffusion used in other fields.[3] It is not until Section V that he develops his argument about the emulation of pioneer states and cue taking from neighboring states. So I believe my interpretation to be a reasonable firsthand one, but I stand corrected. Walker says this diffusion process "can be visualized as a succession of spreading ink-blots on a map."[4] I recall trying a rough approximation of this technique—coloring a map for each of twelve laws—and found no discernible regional variation for time of adoption among my data.

One disagreement I have is with Walker's suggestion at several points that one should remove the South for purposes of testing the model. This line of thinking is offered all too frequently in the state politics literature; to throw out the deviant, and hence the interesting, cases really begs the question of explanation. In one exercise Walker suggests this procedure because he believes the removal of the southern states would make the random mixing assumption more realistic.[5] However, the problem he finds does not stem from this "unrealistic" assumption of the model but from the fact that the civil rights laws have not yet completely diffused, and thus the data for adequate validation are not at hand.

In the second exercise where Walker suggests this procedure he fails to understand my section "Analysis of First Adopters."[6] In this section I found that the first ten states to adopt any particular law were usually more politically competitive and wealthier than the nationwide average in the year of adoption. It is important to note that after ranking and scoring the states according to time of adoption, with many southern states falling at the bottom of the scale, I analyzed only the first ten states to adopt; these, usually, were not southern states. Hence, my finding is not a "statistical artifact" resulting from a failure to treat the South as a separate group; my analysis does not rest at all upon those cases.[7] But as a matter of idle speculation, just how would one go about rank ordering states on relative time of adoption to obtain an ordinal score for the purpose of further statistical analysis and at the same time leave out a quarter of the states making up the ranking for this particular political system?

One final technical point of misunderstanding between Walker and myself concerns my use of rank-order correlations to get at the underlying structure of "innovativeness." I agree with him that my finding of little interrelatedness among twelve issues does not imply that we abandon research on "innovativeness." But my findings do constitute, I think, a caveat that one ought not to assume, a priori, that "innovativeness" is a stable factor; rather, one must establish the existence of "innovativeness" before he seeks to explain it. Walker does not sufficiently validate his innovation scale before he uses it.

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[1] Among those few studies are Edgar C. McVoy, "Patterns of Diffusion in the United States," *American Sociological Review* (April, 1940), pp. 219–227; Thomas M. Scott, "The Diffusion of Urban Governmental Forms as a Case of Social Learning," *The Journal of Politics* (November, 1968), pp. 1091–1108; Robert L. Crain, Elihu Katz, Donald B. Rosenthal, *The Politics of Community Conflict* (Indianapolis: Bobbs-Merrill, 1969), particularly chapter 2.

[2] Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations: A Cross-Cultural Approach* (New York: The Free Press, 1971).

[3] Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 882.

[4] Jack L. Walker, "Comment: Problems in Research on the Diffusion of Policy Innovations," *American Political Science Review*, 67 (December, 1973), 1186–1191.

[5] Walker, "Comment," p. 1188.

[6] Walker, "Comment," p. 1188.

[7] Walker, "Comment," p. 1188.

Fundamental Disagreements

Our disagreement here is rather like that over a half-glass of water, whether it is half full or half empty. Walker asserts that because of my fundamental assumption that the population is completely intermixed I have had to give away much explanatory power.[8] On the contrary, I think I have bought much explanatory power by beginning with one simple model based upon one "unrealistic" assumption. As Milton Friedman says, "the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic,' for they never are, but whether they are sufficiently good approximations for the purpose at hand."[9]

Many other diffusion theorists have considered this assumption, unrealistic though it may be, to be sufficient at first. Both Bartholomew's birth process model for the diffusion of news and rumor and Coleman's social structure model begin with this assumption and then go on to more complex and more realistic theoretic models by relaxing this assumption.[10] However, these more complex models quickly become intractable when their empirical validation is attempted. For this reason I retained my simple model; certainly the assumption does not make New York interchangeable with Mississippi; rather it represents formally a single theory which attempts to explain both states' innovations.

Furthermore, Walker fails to understand that the theoretic assumption of a completely intermixed population (for the sake of simplicity of the model) is not wedded to whether the distributions of adoptions over time are normal. As I mentioned, many other theories can be constructed to fit this curve and their formal representation in a mathematical model may be simple or quite complex, e.g., allowing for constant source diffusion, within-group but not between-group interaction, and disadoption.

I elected to leave the model in its simple state and to work in the direction pointed out by failures of the empirical model to yield accurate predictions—toward developing a political explanation for Mothers' Aid, for example. When one uses this method, beginning with individual issues and not their aggregations, which is far from a "case study approach" as Walker calls it, the likelihood is greater, I think, for the emergence of patterns among states, issue areas, and other phenomena of interest to political scientists than when one begins his study by aggregation.[11]

Direction of Future Research

I agree entirely with Walker that research is needed both on the macro level (the nature of the diffusion process of innovations) and on the micro level (the behavior of adopters as political actors). Also it is critical to establish the bounds of the universe to which our findings are generalizeable. A related problem is that of clarifying the operational definition of an innovation. I have relied on the data source in each case which reports, usually in a single table, when states adopted some law. Over time, however, as the language begins to diverge (as in the old game of "Gossip") the last adopter might not be really innovating in exactly the same manner as the first adopters. One could, of course, compare the language of the two bills from the states' legal codes, not an insurmountable task unless the number of innovations being studied is large.

Finally, I agree with Walker that a theory such as mine based on user interaction obviously cannot account for the first adopter; similar theories, however, have been applied in varying degrees of rigor with success to a range of social phenomena. This is usually taken to be an indicator of the generality of a theory—it can explain what seems to be a wide variety of behaviors. Another virtue of a good theory is that it is consonant with, or can subsume, other theories within the same paradigm. Thus, I, in contrast to Walker, can see no intellectual reason why a theory of diffusion which is parsimonious, powerful (in its predictive ability here), and general cannot merge with appropriate theories of the origin of innovations in a social system.

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[8] Walker, p. 1187.

[9] Milton Friedman, *Essays in Positive Economics* (Chicago: The University of Chicago Press, 1953), p. 15. More strongly, Friedman asserts that "truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)," p. 14.

[10] David J. Bartholomew, *Stochastic Models for Social Processes* (London: John Wiley & Sons, 1967), p. 206; James S. Coleman, *Introduction to Mathematical Sociology* (London: Free Press of Glencoe, 1964), chapter 17.

[11] Walker, "Comment," p. 1189.

Economic Growth and Intergenerational Change in Japan

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Introduction

During the 1960s, comparative politics became concerned with the problems of the developing areas, leading to a proliferation of books and articles on what are now familiar themes—political development, modernization, and nation building. In the 1970s, interest in these themes will no doubt continue, but at the same time problems relating to the advanced industrial societies will probably command increasing attention. A case in point is a recent study by Ronald Inglehart on intergenerational change in the industrial societies of Western Europe.[1]

The question of what happens to politics when industrialization and economic growth produce affluence is both intriguing and important. For a time it was believed by some, for instance Daniel Bell, that affluence would lead to an "end of ideology." Recent events, such as the rash of student revolts and the rise of New Left protest movements in various countries, have cast doubt on the validity of that thesis. In this context, Inglehart's study is revealing and provocative, for it suggests, on the basis of survey research data, that affluence might lead in time to a restructuring of political loyalties in Europe.

Professor Inglehart develops his thesis by integrating theory and empirical data. His theory rests on the idea derived from the late Abraham Maslow's writings on human motivation that individuals have a hierarchy of values, "giving maximum attention to the things they sense to be the most unsatisfied needs at a given time."[2] According to this theory, human beings first must seek to fulfill subsistence needs—food, water, shelter—in short, economic security. When this basic need is met, individuals then seek higher values, namely sense of belonging, followed by self-esteem, and intellectual and aesthetic fulfillment. A further assumption is that individuals tend to maintain a given hierarchy of values "once a basic character has been formed during childhood and youth."[3] It would follow from this that the milieu in which a person has been socialized is important.

The most dramatic change in milieu that has taken place, according to Inglehart, is the rapid economic growth leading to affluence which nations in Western Europe have come to experience in the postwar era. Today's youth "have been socialized during an unprecedentedly long period of unprecedentedly high affluence. For them, economic security may be taken for granted, as the supply of water or the air we breathe once could."[4] The result is a generation gap: the older generation, having been socialized in a period of scarcity, clings to acquisitive "bourgeois" values, while the younger generation gives priority to "postbourgeois" values "relating to the need for belonging and to aesthetic and intellectual needs."[5] Inglehart foresees, moreover, that as the younger generation takes over during the next decades, either new political parties responding to value cleavages might emerge, or there might be a realignment of the social basis of existing parties. Specifically, in the past the middle class has tended to support conservative parties, whereas the working class has preferred the Left parties; but in the future the youth from middle-class families with postbourgeois values may support Left or New Left parties, while individuals from working-class families who have only recently acquired a taste for prosperity may become potential recruits for conservative parties because they will seek to defend or extend their recent gains.[6]

To test this theory of intergenerational change, Inglehart utilized empirical data drawn from Western European nations, namely, Great Britain, France, West Germany, Belgium, the Netherlands, and Italy. He found that since Britain had industrialized early, the relative size of the population with postbourgeois values was largest there, but also that other countries which had a higher economic growth rate than Britain in recent years, notably Italy and France, had experienced the largest amount of intergenerational change in value priorities.[7]

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[1] Ronald Inglehart, "The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies," *American Political Science Review*, 65 (December, 1971), 991–1017.

[2] Inglehart, p. 991.

[3] Inglehart, p. 991.

[4] Inglehart, p. 991.

[5] Inglehart, pp. 991–992.

[6] Inglehart, p. 992.

The Japanese Case

One of the hazards of studying the political effects of industrialization is that historically it has been associated with Western nations. This raises the possibility that one's findings with respect to the consequences of industrialization and economic growth may be greatly influenced by Western history, society, and culture. Thus we need to introduce "controls" for historical, social, and cultural variables. One way to do this would be to look at an industrialized country that is not Western; Japan, which today is a leading industrialized power, is an obvious choice. One objection that could be raised is that Japan is not entirely free of Western influences. Undoubtedly Japan has become Westernized to a certain degree; nevertheless, it is also true that in value preferences and in interpersonal relations, the Japanese have managed to retain much of their own traditions. Most people would probably agree that an examination of the Japanese experience should afford us a better perspective in trying to understand what is going on in the nations of Western Europe.

Another advantage of looking at Japan is the availability of the kind of empirical data not easily found elsewhere. Professor Inglehart noted, for example, that he was studying changes in value priorities that have occurred over a long period of time, but that since relevant time-series data were not available he could not test his hypotheses directly.[8] It so happens that the Institute of Mathematical Statistics, a government-supported but autonomous organization located in Tokyo, has conducted a series of national surveys bearing on national character at five-year intervals beginning in 1953, and repeated in 1958, 1963, and 1968.[9] Thus we have access to a longitudinal study that spans the period of economic growth, namely the late 1950s and the decade of the 1960s. So in a way we can test directly some of the effects of economic growth (the independent variable) on changes in value priorities in Japan.

Of course, the ideal situation would be to replicate the European study, but this is not feasible. So from the long list of questions that were asked in the national character surveys, a small number was chosen that appeared to get at some of the same value priorities that concerned Professor Inglehart. Particular attention has been paid to responses to questions that were asked in 1953 and in 1968 of respondents in two age groups, 20 to 24, and 50 to 54 years old. By so doing, two periods of time separated by fifteen years (during which economic growth took place), and two groups of individuals who were born 30 years or roughly one generation apart can be studied.

The basic assumption is that our samples should include sets of individuals with rather different life experiences. The 20- to 24-year-olds in the 1953 survey were born between 1929 and 1933. Therefore the assumption is that they must have gone through their early childhood during a period of economic depression, experienced early schooling in an atmo-

Table A. Sample Size of Age Groups in Four Surveys

Age Group	1953	1958	1963	1968
20–24	423	201 149	335	376
25–29	345	199 127	361	398
30–34	257	202 128	367	399
35–39	247	146 91	328	389
40–45	221	161 96	279	344
45–49	187	120 89	239	243
50–54	201	107 58	252	222
55–59	135	101 64	152	205
60–64	82	92 43	155	192
65–69	81	65 37	113	128
70–	63	55 38	117	137
Total	2,242	1,449 920	2,698	3,033

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[7] Inglehart, pp. 999–1000.

[8] Inglehart, p. 993.

[9] The national character survey data cited in this article were drawn from Tokei Suri Kenkyujo, *Nihonjin no Kokumin-sei* (Tokyo: 1961) and *Dai-2 Nihonjin no Kokumin-sei* (Tokyo, 1970). For the 1968 national survey a sample of 4,000 respondents was drawn. Metropolitan, urban, and rural areas were stratified in terms of population, region, and occupation. One election district was chosen from each of the areas, the probability of selection being proportionate to the size of the population. Respondents were chosen on a random basis from voter lists, with the size of the sample proportionate to the size of the population. From the 4,000 in the sample, 3,033 interviews were completed. The size of the sample for the four surveys according to age groups is given below. Two sets of figures are given for the 1958 survey. For Tables 1–5, use figures in the lower row; and for Tables 8–9 and 12–13 take the total of both rows.

sphere of increasing authoritarian and militaristic repression, suffered deprivation brought about by severe shortages of food and other necessities during the latter part of the war, and felt the dislocation—economic, social, and psychological—wrought by the trauma of defeat and subsequent military occupation by the victorious Allied Powers. This was a generation obviously buffeted by severe winds of change, and therefore one that might conceivably value economic security rather highly. Those in the 50 to 54-age group, by contrast, were born about 1900 and so went through the Depression and World War II as mature adults.

The 1968 sample, of course, focuses on individuals born between 1944 and 1948, and except for those born in 1944, it would contain the postwar generation. One presumes that in terms of early childhood experiences, they were probably not well off, because the early years of the postwar era were characterized by considerable economic dislocation; but it is also true that as they grew older, the economy also grew, so that by the time they reached their late teens there was a labor shortage and jobs were plentiful. The hypothesis is that this generation would be more likely to prefer postbourgeois values, while the older generation ought to be in favor of acquisitive values. Is the same kind of generation gap to be found in Japan as in Western Europe? I now turn to the Japanese data.

One of the questions in the national character surveys had to do with "goals in life." Respondents were given six choices and asked to pick one as their major life goal:

(1) Work hard and get rich.

(2) Study hard and make a name for yourself.

(3) Don't think about money or fame; just lead a life that suits your own tastes.

(4) Live each day as it comes, cheerfully and without worrying.

(5) Resist all evils in the world, and live a pure and just life.

(6) Never think of yourself; give everything in the service of society.

The first two questions in the list would appear to tap personal achievement values, while questions three and four are related to self-fulfillment and self-gratification. The final two, by contrast, point in the direction of "do-goodism." The responses from the four surveys are tabulated in Table 1:

What the table suggests is that during the fifteen years covered by the surveys, personal achievement values have remained stable, while the self-fulfillment and self-gratification priorities have risen. At the same time, lower value priorities are accorded to leading an exemplary life or working for the good of society.

We can get a better perspective on what is happening to these values by taking questions (1) and (3), which in my estimation roughly correspond to Inglehart's acquisitive and postbourgeois values, and breaking the data down by age groups. Table 2 shows changes in the value of getting rich over the 15-year period:

As Table 2 shows, the 20 to 24-age group has not become less acquisitive over the years, which is to say that affluence does not appear to have affected them. If this age group is compared with the older generation, that is, those in the 50 to 54-age group, it is clear that the older generation is somewhat more acquisitive and that the gap existed as early as 1953, well before the period of rapid economic growth.

Table 2. Age and Percentage Assigning Highest Value to Working Hard to Get Rich, 1953–68

Age	1953	1958	1963	1968
20–24	9%	12%	12%	13%
25–29	12	13	16	14
30–34	11	24	21	19
35–39	15	20	18	21
40–44	19	24	19	16
45–49	20	20	15	17
50–54	17	10	20	22
55–59	16	17	24	18
60–64	15	14	18	17
65–69	27	22	13	18
70–	27	10	13	11
Average	15	17	17	17

Table 1. Goals in Life, 1953–68

Year	Get rich	Fame	Suit one's tastes	Live cheerfully	Live pure	Work for society	Other; DK	Total	(N)
1953	15%	6%	21%	11%	29%	10%	8%	100%	(2,254)
1958	17	3	27	18	23	6	6	100	(920)
1963	17	4	30	19	18	6	6	100	(2,698)
1968	17	3	32	20	17	6	5	100	(3,033)

Table 3. Cohort Analysis of 20–24 & 50–54-Age Groups on Value of Getting Rich

Age Group	1953	1958	1963	1968
20–24 (50–54)	9% (17%)			
25–29 (55–59)		13% (17%)		
30–34 (60–64)			21% (18%)	
35–39 (65–69)				21% (18%)

Actually what may be operating here is life-cycle change. Youth still unburdened by family obligations feel that money is less important, but as they grow older they become more acquisitive out of necessity. Indeed cohort analysis of the data suggests this. In Table 3 are tabulated responses in successive surveys from the same age groups over the 15-year span.

The table shows that 9 per cent of the sample of 20- to 24-year olds in 1953 responded affirmatively to the question of working hard to make money, but that 15 years later the figure from that same age group had risen 12 percentage points. By contrast, the older generation stayed virtually the same.

A rather different profile emerges from question (3), which rejects seeking money or fame and favors instead leading a life that suits one's own tastes. The responses are tabulated in Table 4:

Table 4. Age and Percentage Assigning Highest Value to Leading a Life that Suits One's Tastes, 1953–68

Age	1953	1958	1963	1968
20–24	34%	38%	45%	51%
25–29	23	34	38	42
30–34	29	36	33	38
35–39	21	22	31	31
40–44	15	23	30	37
45–49	14	21	23	28
50–54	14	21	22	23
55–59	12	19	25	19
60–64	9	19	20	19
65–69	10	3	17	16
70–	5	10	12	11
Average	21	27	30	32

Again there is a generation gap, for as early as 1953, the 20- to 24-age group chose this value more frequently than the 50- to 54-year-old generation. But it is also evident that the proportion of youth responding affirmatively has risen with every survey, from 34 per cent in 1953 to 51 per cent in 1968. This suggests the possibility that long-term intergenerational change has been occurring. This interpretation appears to be confirmed by a cohort analysis of changes in the younger and older generations summarized in Table 5:

As will be noted, the percentage of those responding affirmatively has not changed very much among these age groups over the 15-year period. That is, the change that has occurred is mostly accounted for by each new age group's acquiring somewhat different value priorities. Thus, as the older generation dies off, there is a progressive shift in values in the direction, in this case, of self-fulfillment.

Another way of looking at acquisitive vs. postbourgeois values is provided by a question that states: "In bringing up children of primary school age, some people think that one should teach them that money is the most important thing. Do you agree or disagree?" The responses obtained in three surveys across all age groups is presented in Table 6:

In terms of the overall average, the majority of respondents agree that children ought to be taught that money is most important. There is some difference, however, between the 20 to 24 and the 50 to 54-age groups. The older generation is somewhat more acquisitive and has been as far back as 1953. But the percentage of respondents who agree with the question has

Table 5. Cohort Analysis of 20–24 & 50–54-Age Groups on Value of Leading a Life that Suits One's Tastes, 1953–68

Age Group	1953	1958	1963	1968
20–24 (50–54)	34% (14%)			
25–29 (55–59)		34% (19%)		
30–34 (60–64)			33% (20%)	
35–39 (65–69)				38% (16%)

Table 6. Age and Percentage Assigning Highest Value to Teaching Children that Money is Most Important, 1953–68

Age	Agree			Disagree		
	1953	1963	1968	1953	1963	1968
20–24	60%	43%	34%	31%	36%	50%
25–29	66	55	49	28	26	36
30–34	63	58	58	22	23	27
35–39	62	56	59	21	23	26
40–44	65	63	59	27	19	26
45–49	66	62	62	21	22	24
50–54	72	68	65	18	20	18
55–59	72	72	67	19	16	19
60–64	77	66	66	15	23	20
65–69	78	72	73	12	15	14
70–	78	66	60	11	20	14
Average	65	60	57	24	23	28

Table 7. Cohort Analysis of 20–24 & 50–54-Age Groups on Teaching Children that Money is Most Important, 1953–68

Age Group	Respondents Agreeing		
	1953	1963	1968
20–24 (50–54)	60% (72%)		
30–34 (60–64)		58% (66%)	
35–39 (65–69)			59% (73%)

been declining somewhat over the years (from 65 per cent in 1953 to 57 per cent in 1968); and the change is especially marked among the young group, which suggests that intergenerational change may also be at work here. Again cohort analysis of these two age groups confirms the impression, for as Table 7 shows, samples from the same age group over a period of time show almost no change.

What about the espousal of postbourgeois values, which, according to Professor Inglehart, characterizes youth in Western Europe? The Inglehart survey tried to measure postbourgeois values by scoring responses to two items, namely, "Giving the people more say in important political decisions," and "Protecting freedom of speech." Unfortunately, the Japanese national character surveys do not contain questions that get at exactly the same value dimension. Two questions, however, at least point in

Table 8. Age and Willingness to "Leave Everything up to" Political Leaders, 1953–68

Age	Agree				Disagree			
	1953	1958	1963	1968	1953	1958	1963	1968
20–24	30%	26%	22%	22%	54%	60%	58%	63%
25–29	33	31	24	25	48	50	54	58
30–34	41	32	24	30	46	51	52	52
35–39	44	35	27	26	38	45	46	54
40–44	52	34	24	25	28	46	51	57
45–49	51	31	29	34	29	46	50	49
50–54	52	46	33	29	26	34	40	50
55–59	51	42	36	34	28	27	40	45
60–64	51	37	47	43	19	29	30	35
65–69	57	50	35	39	15	18	34	33
70–	70	43	45	46	6	19	30	27
Average	43	35	29	30	38	44	47	51

Table 9. Cohort Analysis of 20–24 & 50–54-Age Groups on "Leaving Everything up to" Political Leaders, 1953–68

Age Group	Respondents Agreeing 1953	1958	1963	1968
20–24 (50–54)	30% (52%)			
25–29 (55–59)		31% (42%)		
30–34 (60–64)			24% (47%)	
35–39 (65–69)				26% (39%)

the same direction. The first one has to do with who should make political decisions. The question reads: "Some people say that if we get good political leaders, the best way to improve the country is for the people to leave everything up to them, rather than for the people to discuss things among themselves. Do you agree or disagree? Table 8 summarizes the replies:

The data show that the younger generation is much more reluctant than their elders "to leave everything up" to their political leaders. Again the gap is evident as early as 1953, but interestingly enough, it has narrowed over the years. There was a 22-percentage point difference in 1953, but only 7 percentage points in 1968 between the 20- to 24-year olds and the 50- to 54-year olds. What appears to be happening here is neither intergenerational nor life-cycle change, but rather adult change. This is a shift in attitudes in a certain direction among the adult population over a period of time. This is borne out by Table 9, which presents a cohort analysis of the two generations:

As Table 9 shows, the percentage of those among both the younger and older generations who agree to leave everything up to political leaders has in general declined somewhat.

The second question which would appear to be related to postbourgeois values has to do with the socialization of the young. "In raising children of elementary school age, do you think it is more important to teach them to value freedom or discipline?" Table 10 summarizes the results:

Since this question was asked only in 1953 and again in 1968, we cannot follow the change (if any) that occurred in the intervening years. It is evident, however, that the majority of respondents, young and old, favor discipline over freedom. Japanese youth, in this sample at least, do not appear to be very rebellious. Still, proportionately more youth favor freedom than do older people, for in 1968, 34 per cent of the 20 to 24-year old group favored freedom in contrast to 17 per cent for the 50 to 54-year olds. Although there is not enough data to do cohort analysis, an examination of Table 10 suggests that this may represent a case of life-cycle change. For instance, 25 per cent of those in the 20- to 24-age group in 1953 favored freedom, but a sample from that same age group 15 years later revealed that only 16 per cent favored it. The opposite is true with those who favor discipline. In 1953, 56 per cent favored discipline, but by 1968 respondents in that particular group favored discipline even more strongly, registering a 73 per cent affirmative response. It appears that as people get older they favor freedom less and discipline more.

Table 10. Age and Percentage Favoring Freedom vs. Discipline in Raising Children, 1953, 1968

1968 Age Group	 Discipline	 Freedom	1953 Age Group	 Discipline	 Freedom
20–24	57%	34%	20–24	56%	25%
25–29	63	27	25–29	64	17
30–34	68	21	30–39	60	14
35–39	73	16			
40–44	74	12	40–49	69	14
45–49	72	14			
50–54	73	17	50–59	66	11
55–59	75	15			
60–	66	15			
Average	68	20	Average	63	16

To summarize, Japan's youth, when compared to their elders, appear to be less acquisitive, more democratic, and somewhat more inclined to value freedom; in short, they show signs of preferring the kind of values defined by Inglehart as postbourgeois. Closer examination of the rather limited data suggests, however, that three kinds of value changes may be occurring: (1) intergenerational change, (2) lifecycle, and (3) adult. In the case of acquisitive values, there is a mixed picture. Responses to one question hint at intergenerational change, but another question suggests life-cycle. The same thing may be said of postbourgeois values. The question on living the kind of life that suits one's own tastes elicited responses that suggested intergenerational change; but the question on leaving everything up to political leaders implied that adults had been changing their values in a more democratic direction. In short, the view an observer gets of value changes in Japan is not so neat and tidy as the one Inglehart presents on Europe. I can only speculate about why this should be the case. More cross-currents may be operating in Japan because of the juxtaposition of tradition and modernity in Japanese society and culture. The greater complexity may also be an artifact of the kind of data available to the political analyst. If Professor Inglehart had had available to him the kind of longitudinal data I have used, he might have come out with a somewhat more complicated and therefore less tidy picture of the value change that had been going on in Europe.

Changes in Political Partisanship

What is the relationship between value changes and political partisanship? Inglehart believes, it will be recalled, that shifts in the social basis of parties may well occur in Western Europe during the next 20 years as a result of intergenerational change in values that has occurred and presumably will continue. Specifically, he suggests that "the presence of postbourgeois values is linked consistently with a relative tendency to remain loyal to the Left, among those who were brought up in that tradition, and with a tendency to shift to the Left among those who were raised in other political climates."[10] Unfortunately the data from the Japanese surveys do not allow this proposition to be tested directly in the Japanese setting.

Indirectly, however, some inferences can be made. If one assumes, as does Inglehart, that education is an accurate indicator of "parental affluence during the respondent's formative years,"[11] then college-educated children, presumably having come from the more affluent families, ought to be the bearers of postbourgeois values. There is some data on the relationship between education and support for the Socialist party over the 15-year period.

Table 11. Education and Support for Socialist Party, 1968

Percentage Supporting Socialist Party

Year	Elementary & Middle School	College
1953	34%	33%
1958	53	36
1963	37	26
1968	40	22

Table 11 shows quite clearly that except for 1958, when the Socialists reached the peak of their popularity, support among those who went only to elementary or middle schools increased slightly. By contrast, support for the Socialist party among college-educated respondents has dropped (except for 1958). This would suggest that the situation in Japan is somewhat different from what Inglehart found in Western Europe.

Another way to look at partisanship is to relate it to age groups. In the past, age has been a good predictor of party support; that is, the young tend to prefer the Socialists and other Left parties, while the elders incline toward conservatism. Table 12 shows the relationship between age and party support:

As can be seen from Table 12, in 1958 there was a 26 per cent spread in support for the Socialists between the group of 20- to 24-year olds and the 50- to 54-year olds. The difference has narrowed in recent years, but the younger age groups generally remain more radical when compared to the older people. Another observation that may be made is that life-cycle change also appears to be present. This is consistent with the conventional wisdom that says that young people tend to be radical but become more conservative as they grow older. In 1953, 41 per cent of the sample of 20- to 24-year olds supported the Socialist party, but the same age group by 1968, now 35 to 39 years old, had dropped to 22 per cent. A third tendency that may be noted from Table 12 is the steady decline in support for the Socialists among youth in their early twenties during the 15-year span. This finding again is consistent with the argument that has been made by numerous journalists and observers that Japanese youth in recent years have become less radical. But in becoming less radical they have not necessarily become pro-conservative. Rather the evidence suggests that in recent years a sizable group of young people have withdrawn from active concern with the electoral process. Responses to questions about intention to vote in national elections are tabulated in Table 13: A general decline in the commitment to vote in almost all age groups may be noted from the data in Table 13, but the decline is particularly steep among the younger age groups. Supporting evidence may be found in other surveys. Since voting is often regarded as a duty rather than as a right of every citizen, election commissions are actively engaged in trying to get as many people as possible to turn out on election day. It is not surprising, given this orientation, that the government should be interested in collecting data by means of surveys of who votes and who does not. According to government statistics, in 1952 about 15 per cent of those in their twenties, it is estimated, did not bother to vote in elections for the House of Representatives, but by 1969, the figure had risen to about 29 per cent, that is had almost doubled.[12] Moreover, in the 1968 national character survey, 31 per cent of the respondents in the 20- to 24-age group reported they supported no party (the highest of all age groups), and since this same group had the lowest percentage of individuals who intended to vote, the inference is that the fall-off in the commitment to vote must be related at least in part to an ebbing of partisan feelings.

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[10] Inglehart, p. 1013.

[11] Inglehart, p. 1005.

There has been some speculation about why Japanese youth have become more apathetic in recent years. Lack of information probably can be dismissed as a primary cause. Generally in public opinion surveys, the percentage of respondents in their twenties who say "Don't know" when asked about their views on international affairs and current political issues is lower than those in the older age groups, who are more likely to vote in elections. Perhaps this is to be expected, since the younger generation is usually better educated than their elders. Undoubtedly, part of the apathy is a reflection of negative sentiments toward the present political system. The prospects of changing the one-party dominant system in which the conservative Liberal Democratic party has held the reins of power for years are not bright, at least in the near future. Thus any person who seeks to use his vote to bring a new group of leaders to power understandably feels frustrated.[13]

It is also conceivable that the decline in electoral turnout reflects a rejection of the parliamentary system. There are some ultra-radical student organizations that nihilistically reject virtually all forms of authority. But the number of young men and women who belong to such organizations is very small and certainly unrepresentative of the great majority of Japanese youth.

It is doubtful that the inability to dislodge the conservatives or rejection of the parliamentary system represent major causes of the fall-off in electoral participation. I am inclined to believe that an important factor is the tendency toward individuation and privatization. A leading Japanese political scientist, Masao Maruyama, has argued that modernization in Japan has increased the desire for "individuation." By this he means that an individual has become "emancipated" from "communal ties which have hitherto bound him and which have pre-

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[12] Aiba, Jun-ichi, "Seinen no Seiji Ishiki," *Shakaigaku Hyoron*, 22 (December, 1971), p. 17.

[13] Aiba, p. 28.

Table 12. Age and Party Support

Age Group	Liberal Democratic				Socialist			
	1953	1958	1963	1968	1953	1958	1963	1968
20–24	30%	31%	27%	29%	41%	40%	32%	25%
25–29	42	37	38	34	28	36	30	25
30–34	32	38	38	35	27	40	27	28
35–39	31	44	40	42	25	24	24	22
40–44	34	41	51	43	21	30	17	25
45–49	39		45	48	18		23	19
50–54	37	44	48	52	15	22	20	17
55–59	37		55	48	8		11	18
60–64	39	37	57	47	7	14	9	15
65–69			50	56			9	14
70–			52	52			4	9
Average	41	38	43	41	23	31	22	22

Table 13. Age and Intention to Vote, 1953–68

Age Group	Will Vote			Will Try to Vote			Disinterested		
	1953	1963	1968	1953	1963	1968	1953	1963	1968
20–24	54%	39%	34%	40%	55%	58%	4%	6%	7%
25–29	59	47	40	36	48	53	5	4	6
30–34	63	55	41	34	40	53	3	5	6
35–39	62	51	56	34	41	42	4	7	2
40–44	62	60	57	32	35	38	5	5	5
45–49	69	53	60	28	42	37	2	4	2
50–54	66	57	57	26	35	39	7	6	3
55–59	71	59	60	21	38	35	8	1	5
60–64	68	66	59	26	26	33	4	4	6
65–69	63	57	67	26	35	28	10	7	4
70–	55	61	61	23	30	29	16	6	8
Average	62	53	51	32	41	44	5	5	5

scribed to him certain traditional behavior."[14] Robert J. Lifton has described in his study of Japanese youth the concern young people have with the idea of *shutaisei,* which involves having and living by personal convictions, or selfhood, and joining with other like-minded people to achieve historical goals, that is having social commitment. "Their greatest difficulty," he continues, "is in realizing to their own satisfaction its first element, that of selfhood; and the sense of 'smallness' or the 'inferiority complex' which they talk so much about seems to reflect the great difficulty the Japanese have in perceiving and believing in a relatively independent self."[15]

One of the questions in the national character surveys cited earlier having to do with leading a life that suits one's own tastes (Table 4) may well be tapping this dimension, that is the search for selfhood, or individuation. It will be recalled that responses to this particular question indicated intergenerational change.

One of the several ways in which this individuation process can proceed is "privatization," which Maruyama defines as an orientation toward the "achievement of self-gratification rather than public goals" and a "withdrawal" or "retreatism" as a "conscious reaction against the increasing bureaucratization of the system and against the complexities of the social and political process in which he finds himself involved."[16] There is some empirical evidence that privatization may be occurring among sections of youth in Japan.

One of the questions in the 1968 national character survey sought to determine which had higher priority, individual rights or the public interest.

> There are the following opinions, which one do you approve? Of course, it may depend on the situation and degree, but in general which one should be followed? (a) In order to give recognition to one's rights, it may be necessary that the public interest be sacrificed sometimes; (b) an individual's rights may have to be sacrificed sometimes in order to promote the public interest.

The responses are indicated in Table 14:

One might have hypothesized that young people, being more idealistic and less acquisitive, would favor promoting the public interest, but as Table 14 shows, this is not the case. If the 20- to 24-year old group is compared with the 50- to 54-year olds, there is a 17-percentage point difference, with youth favoring individual

Table 14. Age and Individual Rights vs. Public Interest, 1968

Age Group	Individual Rights	Public Interest	Other
20–24	46%	50%	4%
25–29	38	52	10
30–34	42	48	10
35–39	32	59	9
40–44	28	66	6
45–49	33	58	9
50–54	29	60	11
55–59	26	60	14
60–	19	62	19
Average	33	57	10

[14] Masao Maruyama, "Patterns of Individuation and the Case of Japan: A Conceptual Scheme," in *Changing Japanese Attitudes toward Modernization,* ed. Marius B. Jansen (Princeton: Princeton University Press, 1965), p. 494.

[15] Lifton, Robert J., "Youth and History," *Asian Cultural Studies,* No. 3 (October, 1962), p. 125.

[16] Maruyama, p. 498.

rights over the public interest. The older generation was socialized in the prewar period when both the educational system and the official propaganda emphasized the duty of every person to subordinate his welfare to the good of the collectivity, whether it was the family, society, or the state. The younger generation, raised in a more democratic political atmosphere and educated in a school system that no longer emphasizes service to the state, appears to put higher priority on personal welfare and self-gratification.

In the absence of more detailed data, one can only speculate about why some Japanese youth should favor privatization. It is certain that economic pressures on youth lessened considerably in the 1960s, thanks to the high economic growth rate. The business boom, fed by rising exports as well as domestic consumption, led to persistent labor shortages. Jobs became more plentiful, and although prices were pushed upward by the inflation, wage scales rose even faster. All of this enabled Japanese youth to indulge themselves and to engage in the pursuit of pleasure and self-gratification. By contrast, the older generation, still bound by the traditional ethic that stressed individual sacrifice for the collectivity, continued to take its social responsibilities more seriously.

Conclusion

Since the available data are rather sketchy and not sharply focused on the problem at hand, one must be circumspect about drawing sweeping conclusions from them. It can be said, however, that different kinds of change—intergenerational, life-cycle, and adult—appear to be at work and that the general picture is less clearcut than the one Inglehart paints for Western Europe. I am inclined to believe that in Japan there is less a shift from acquisitive to postbourgeois values than a long-term change from a collectivity orientation to individuation. Indeed, the most parsimonious explanation for much of the data would be the trend toward individuation. For example, a shift away from acquisitive values may represent a weakening of the family as a cohesive unit. In the past the pressure for individual achievement usually came from parents who were eager that their children achieve fame and fortune because, among other things, this would help elevate the social standing of the whole family. I do not wish to imply that this is no longer the case, but it is true that family cohesion is weakening in the face of modernization. Likewise, the decline in willingness to take the trouble to vote in national elections may be taken as a sign that community pressures are abating. Much of the time, voting may represent more an act of group conformity than an expression of one's political convictions. Finally, privatization, as has been suggested, may represent a form of individuation.

In any case, the Japanese data would seem to cast a shadow of doubt on the theory of a hierarchy of values which Professor Inglehart invokes to explain his thesis. Following Maslow, he argues that man's first goal is to meet his physiological needs and when he has attained them, he turns to social needs, followed by needs for self-esteem. Some critics have questioned the usefulness of this formulation in explaining human behavior and have also asked whether we are to "assume that the needs operate in all cultures and situations, or only in some."[17] It is my contention that in Western culture, which has long stressed individualism, youth may seek a sense of belonging, whereas in Japanese culture, which has emphasized the group, youth may yearn for individuation and privatization.

What implications all this has for the future of Japanese politics is unclear. There are still no clear signs that new parties may emerge or that a realignment of the social basis of old parties may take place to reflect new cleavages in value priorities. If anything, many young people appear to be turning away from politics. This may be a short-run phenomenon, and a period of more active involvement in politics may yet appear, just as the political apathy found in many American campuses in the 1950s was replaced by the turmoil of the 1960s. At the very least, the direction of change in party loyalties becomes difficult to predict. There is now an element of uncertainty, portending that the political stability and incremental change characteristic of postwar politics in Japan may in time come to an end.

Finally, the Japanese data here analyzed calls into question the validity of generalizations about the political consequences of industrialization based solely on data drawn from Western nations. The rise of Japan to a status of a leading industrial power means that no one interested in the problem of the effects of industrialization and economic growth on politics can afford to ignore the Japanese experience.

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[17] Silverman, David, *The Theory of Organizations: A Sociological Framework* (London: Heinemann, 1970). pp. 81–82.

Issue Area and Foreign-Policy Process: A Research Note in Search of a General Theory

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Is there a foreign-policy process? How does foreign policy making differ from policy making in the domestic domain? What is the impact of process on policy outcome? This note attempts to further our efforts to answer these questions by suggesting a general paradigm for the study of the foreign-policy process. The paradigm is explicitly synthetic; it combines insights generated by an early contribution of Arnold Wolfers to our understanding of the linkage between foreign-policy behavior and domestic political process with those prompted by two recent efforts by Theodore Lowi to affirm the nexus between issue and policy process. The paradigm, it is argued, makes it possible to specify the nature of the policy process (the identity of the major actors, the intensity of conflict) which will obtain in reasonably integrated modern states for a given case in point. It permits us to specify the social science literatures which are most germane to the generation of insights pertaining to the nature of the policy outcome. Finally, it gives us a framework within which it will be more possible to answer the old question about case studies: "Of what is this a case?"

The Pole of Power and the Pole of Indifference

Wolfers's essay[1] is largely devoted to ascertaining when in the name of analytic parsimony is it justifiable to disregard the impact of the internal workings of the state on foreign policy behavior. When, in other words, may one confidently expect that "black boxing" the state will serve the scientific goal of prediction? His answer is based on several propositions. It depends, he asserts, on whether the decision makers acting in the name of the state attach primary loyalty to the state and on whether, similarly, states are the sole corporate actors in the international environment.

The most relevant aspect of Wolfers's argument for the concerns of this article is his implication that a crucial determinant of the policy process is the nature of the issue at stake. He develops the relation of issue to policy process by asserting the existence of two poles—the pole of power and the pole of indifference —which may be conceived as end points of a continuum measuring the hierarchy of values of participants in the political process. With regard to the pole of power, Wolfers's analogy to individual behavior when someone yells "fire in the hall" should be familiar to all students of foreign policy. The preservation of the state is a goal highly esteemed by almost all decision makers. Consequently, a direct, profound threat to the existence of the state is in most instances likely to result in power maximizing behavior of the kind anticipated by those in the states-as-sole-actors tradition. What is frequently forgotten is Wolfers's suggestion that it is equally plausible to assume a predictable result in situations where "danger and compulsion are at a minimum. When not more than minor values are threatened by international discord, governments usually find it expedient to act according to established rule, since their interest in seeking others to do likewise exceeds their interest in winning an occasional and minor advantage"[2]—and hence one expects a high incidence of compliance with diplomatic protocol even by new states which have no part in establishing the rules. Thus, according to Wolfers, when events approximate either end of the continuum it is appropriate to expect that unitary, rational decision-maker models of state behavior will provide a close fit with reality. Under such circumstances, analysis of policy process or the perceptions of the decision makers would be reserved for the occasional deviant case.

Wolfers's analysis concedes too much to those who disregard the internal workings of the state or the minds of those who act in its name. It is by no means clear that in the atomic age the response of all decision makers to the functional equivalent of fire in the hall will be similar and highly predictable. Wolfers observes that "in international politics, the house is not always, nor everywhere, on fire although the temperature may not be comfortable even under the best of circumstances."[3] What he fails to note is that in international politics, even when the house is on fire, staying within the house may be preferable to going outside. The divergent exhortations "Better red than dead" and "Better dead than red" symbolize the extremities among a wide range of alternatives which *might* be adopted by U.S. decision makers when core values are threatened. The range of responses to the Soviet missiles in Cuba in 1962 actually advocated by members of Kennedy's entourage lends further support to stressing the severely limited predictive power of models which disregard microanalytic phenomena, even under crisis conditions, when applied to superpower behavior.[4] The response of lesser powers to armed intervention by a superpower is likewise much affected by microanalytic variables: The propensity to fight if attacked—as the Yugoslavs since 1968 have repeatedly told themselves and others—appears greatly dependent on national culture and level of development. Even in crisis situations one disregards the perceptions of decision makers—and the elements which shape perceptions—at one's peril. We have progressed too far in our awareness of the impact of organizational structure and small-group dynamics on policy outputs not to include these in assessments of policy outcomes.

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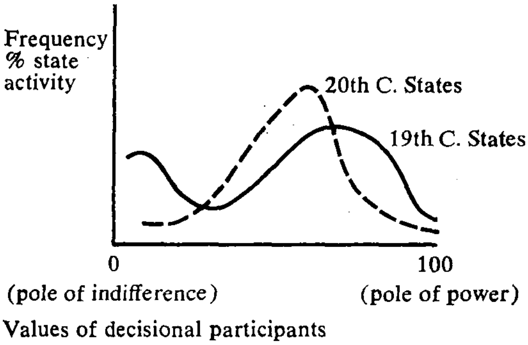
[1] Arnold Wolfers, *Discord and Collaboration: Essays on International Politics* (Baltimore: The Johns Hopkins Press, 1962), pp. 3–35. Harold K. Jacobson reminds me that Wolfers was frequently less disposed than in "The Actors . . ." to accord predictive power to rational models, even at the pole of power—hence his emphasis on "National Security as an Ambiguous Symbol," *Discord and Collaboration: Essays on International Politics*, pp. 147–65.

[2] Wolfers, *Discord*, p. 16.

These caveats notwithstanding, Wolfers's argument is highly suggestive. It intimates an important explanation for the durability of the "billiard ball" model of international politics. Autocracies and republics alike in the nineteenth century were much more restricted in the scope of their activities than are their twentieth-century counterparts. Given the norm of laissez faire, the bulk of state actions in international affairs was concentrated precisely on the tasks of protection and the facilitating of interaction between states (the exchange of information and diplomats) and, in the mid-nineteenth century, regularizing the exchange of mail and telegraphic dispatches between individuals of various states. In terms of Wolfers's continuum, nineteenth-century state activity in international affairs was bimodally distributed. By contrast, twentieth-century liberal democracies and modern dictatorships alike extended the scope of their activities. For twentieth-century states the preoccupations of decision makers have been more normally distributed along a continuum reflecting the values of the relevant internal actors. A reasonable graphic aggregation of such states' behavior might look like a normal curve skewed somewhat toward the pole of power (Figure 1). For events located at the poles of power and indifference, the analysis of domestic politics *as such politics is commonly understood* may be largely disregarded. (The qualification is an important one; I am not suggesting that, for instance, politics is absent in crisis decision making—only that the participants in the process act as individuals rather than as spokesmen for groups, institutions, or major strata of society.) For these events it suggests a rationale—aside from the presumably asymmetrical impact of the external environment—for the study of foreign policy separate from the study of "domestic" political processes. A corollary, furthermore, is that there exists an enormous range of events between the two poles for which domestic politics as normally understood is immensely relevant. In the large majority of "foreign policy" cases the analysis of policy process is of major concern.



Distribution of State Activity

Figure 1.

Arenas of Power

It is the existence of policy processes which alter as a function of issue area[5] ("arena of power" in Lowi's terminology) which constitutes Lowi's fundamental insight.[6] The argument of each article may be summarized briefly. In "American Business, Public Policy, Case-Studies, and Political Theory," Lowi identifies three major arenas of power which, he contends, constitute the major patterns of politics in the United States and, in an aside, excludes foreign policy from his analytical scheme.[7] The three issue-areas he labels the arenas of distribution, regulation, and redistribution. What separates one issue area from another is the extent and manner in which the political goods in a case in point can be disaggregated. Those domestic issues which are highly subject to disaggregation Lowi labels the politics of distribution. "Pork barrel" items typify this issue area; the political goods are readily subject to being parceled out to the major units. Nearly everyone (read, everyone who counts politically) gets—in the contemporary argot—a piece of the action.

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[3] Wolfers, p. 15.

[4] Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown, 1971).

[5] Other contributions to the "issue area" literature include several community power studies—Robert A. Dahl, *Who Governs: Democracy and Power in an American City* (New Haven: Yale University Press, 1961); Nelson W. Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); Aaron Wildavsky, "The Analysis of Issue Contexts in the Study of Decision-Making," *Journal of Politics*, 24, (November, 1962), 717–32; and James N. Rosenau, "Pre-theories and Theories of Foreign Policy," in *Approaches to Comparative and International Politics*, ed. R. Barry Farrell (Evanston: Northwestern Univ.

The arena of regulation, by contrast, is the domicile of the group theorists: Coalitions of like-minded groups represent the major patterns of alliance as the classic question of who gets what is decided. In such instances the political goods are not as subject to disaggregation as they are in distributive politics. Where it is often difficult in the distributive arena to identify specific losers, there are always losers in the regulatory arena. "In the short run the regulatory decision involves a direct choice as to who will be indulged and who deprived."[8] Some will not obtain a liquor license in Ann Arbor, just as some will not obtain an overseas air route.

If the arena of regulation is one in which the models of pluralist theorists provide a close approximation to reality, redistributive politics are the domain of elitists such as C. Wright Mills. The actors neither are groups nor are they the firms or individuals typical of distributive politics. Rather, the impact of the event is felt by far larger aggregates—social classes, "peak associations," movements. Since, moreover, the (perceived) relative status and power position of such aggregates is thought to be at stake—the political goods involved being both tangible and nontangible—disaggregation is extremely limited.

In "Making Democracy Safe for the World," Lowi's focus and argument shifts somewhat. Whereas in "American Business, Public Policy . . . ," Lowi dismisses foreign policy in a footnote, in "Making Democracy Safe for the World," his central concern is with the foreign-policy process. His major conclusion is that there is no substance to the view that "foreign policy-making is a fundamentally different policy-making system, or one that is in any considerable way insulated from domestic political

Lowi's Arenas of Power: A Summary

Chart I

Arena	Primary Political Units	Patterns of Interaction	Power Structure	Foreign Policy/ Domestic Policy
I. distribution	individual, firm	log rolling; "unprincipled" alliances of uncommon interests	nonconflicting elite with support groups	both
II. regulation	group	"the coalition;" shared subject matter interest aggregation	pluralistic, multi-centered	both
III. a) protection b) interaction	individuals "without their institutions"	"team norms"	highly elitist	foreign policy
IV. redistribution	classes, movements generations	"peak associations"	conflictual elite: elite and counter elite	domestic

(Borrows heavily from Lowi, "American Business . . . , p. 713.)

Press, 1966) pp. 27–92 and "Foreign Policy as an Issue-Area," in Domestic Sources of Policy, ed. James N. Rosenau (New York: The Free Press, 1967) pp. 11–51.

[6] Theodore J. Lowi, "American Business, Public Policy, Case Studies and Political Theory," *World Politics,* 16 (July, 1964), 677–715; "Making Democracy Safe for the World: National Politics," in Rosenau, *Domestic Sources,* pp. 295–331.

[7] Lowi, "American Business . . ." p. 689, n. 17; "Foreign policy, for which no appropriate '-ion' word has been found, is obviously a fourth category."

[8] Lowi, pp. 690–91.

forces."[9] Instead, he again finds that "three predictable subsystems" may be identified. For foreign policy they are: (1) "an elitist subsystem (although involving a much smaller elite than the 'power elite') that prevails (a) in crisis and (b) in any noncrisis situations in which in the short run no internal resources are involved at all;"[10] (2) "a logrolling pattern . . . similar to . . . [domestic] distributive politics;"[11] and (3) a pluralistic regulatory subsystem in which conflicting groups are the main actors. This time it is redistribution which is relegated to an aside.[12] From the two articles, thus, four separate policy processes are identified. The two Lowi articles are summarized in Chart 1. Each, Lowi concludes, has modal actors and patterns of interaction. Two, distribution and regulation, are useful for both the study of domestic and foreign policy. One—arena III—presumably is unique to foreign policy while the fourth, redistribution, is restricted to the domestic domain.

An Issue-based Foreign Policy Paradigm

One element which serves to link the Wolfers and Lowi pieces is immediately evident. Apparently entirely independent of Wolfers, Lowi has "discovered" the poles of power and indifference. Like Wolfers, Lowi concludes that for events at these two poles, the policy process does not involve the nongovernmental, societal actors, or patterns of interest articulation and aggregation typically found in "domestic" politics. Beyond that, there is a clear ordering in the stakes for which politics is played in the arenas of distribution, regulation, and redistribution—an ordering which relates to Wolfers's continuum. For if the poles of power and indifference are the extremities of a continuum measuring the values of decisional participants, it follows that the arenas of distribution, regulation, and redistribution may be located in ascending order on such a continuum:

(pole of indifference) (pole of power)
0 ———————————————— 100
distribution regulation redistribution

and the policy process for issues which may be properly subsumed under the regulatory interval along Wolfers' continuum would predictably correspond to the conflictual, pluralistic group politics which is the mode of the regulatory arena.

The issue, of course, then becomes by what criteria shall the intensity of the stakes or values at issue be measured. Again the linkage between Wolfers's and Lowi's analysis stimulates an answer: If, in terms of process, the politics of protection and interaction are similar and if the poles of power and indifference do represent the ends of a continuum, then it ought theoretically to be possible to treat that continuum as circular and to identify and label a matrix in which the poles of power and indifference may be appropriately located in the same cell.

Lowi's assertion is that the reason for the similarity between the policy process of crisis politics and that of such foreign-policy acts as recognition is that both have no short-run domestic political consequences.[13] The assertion that crisis politics does not have domestic political consequences seems, however, sufficiently bizarre—in the era of the fifteen-minute war the consequences may be felt in the very short run—to warrant an alternative hypothesis.

The kind of "apolitical" consensual politics associated with protection-insuring and interaction-facilitating policies occurs when the dominant perception of all involved is that the impact of the decision will be *symmetrical* in its impact on the values of citizens. If most citizens are affected in the same way, regardless of the substance of the decision, instances of protection and interaction are directly analogous to distributive politics. In all three instances, decisions have a symmetrical impact domestically. Either the decision has no perceived impact, or there is a threat to the core values of the society, or all the relevant political actors are indulged. Under both regulation and redistribution, by contrast, there are those who are indulged and those denied. An event will be either regulative or redistributive if in the short run its effects are nonsymmetrical in impact.

At the same time, politics in the arena of interaction and protection shares something in common with politics in the arena of redistribution: all these issues involve values which are not confined entirely to tangible resource allocations matters alone. Rather, in the case of all three, nontangible issues are clearly at stake. Interaction issues involve no domestic resource allocation questions. Events properly classed within the arena of protection by their very nature pertain to the core values of the society. Redistributive issues, as we have seen, entail the reallocation not only of tangible goods between classes and other large elements in society but also such nontangible and highly important values as the redistribution of status and power in society. Each contrasts with the arenas of distribution and regulation—the special preserves of resource allocation issues.

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[9] Lowi, "Making Democracy Safe . . . ," p. 314.

[10] *Ibid.*, p. 324.

[11] *Ibid.*

[12] *Ibid.*, p. 325, n. 61: ". . . it is interesting to note the absence of a foreign-policy variant of redistribution. This might be due to my incomplete coverage rather than to a datum about the system."

[13] *Ibid.*, p. 324.

The answers, then, to two questions serve as the basis for a paradigm of foreign-policy processes. These questions are: (a) Is a decision perceived to be symmetrical in its impact on politically relevant domestic political actors? (b) Are the political goods at issue exclusively tangible? (Figure 2). By ascertaining answers to these questions, one should be able to predict the nature of the policy process and to stipulate the social science literature most likely to facilitate the generation of insights relevant to the prediction of outcomes. An affirmative answer to both questions predicts that the issue will be distributive. As such, the primary political units, and the patterns of interaction and power structure are those predicted for the arena of distribution (Type I policy process), and the theoretical literature recommended is largely that of E. E. Schattschneider.[14]

A negative answer to question (a), i.e., the decision is perceived to be asymmetrical, coupled with an affirmative answer to (b), similarly classifies the issue as regulatory (Type II) and informs us that this is an occasion when the usual analysis of domestic politics by the group theorists will be entirely appropriate for a "foreign policy" issue.

By contrast, a positive response to (a) and a negative answer to (b) informs us that rational policy models augmented by a grounding in social psychology—specifically, literature treating the behavior of small groups, information processing by individuals and organizations (e.g., cognitive dissonance), deviant behavior—and a thorough awareness of the ubiquitousness of high politics will be relevant to the prediction of outputs. Similarly, it permits one to assert with some confidence that such traditional societal inputs into the policy process as interest articulation may be dismissed as effectively irrelevant to the outputs for such Type III issues.

Finally, should the answers to questions (a) and (b) both be negative, one would properly conclude that foreign-policy issues may indeed be redistributive and thus to assert that elite theorists and specialists on internal conflict[15] constitute relevant authoritative sources for inspiration.

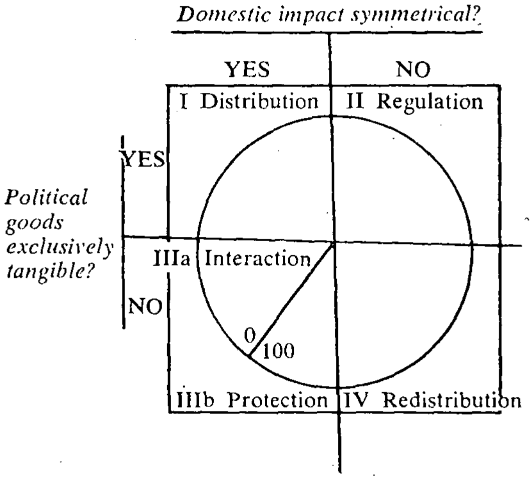

Figure 2. An issue-based foreign policy paradigm.

The Unlimited Politics of Limited War

It is to the fourth type of policy process that we now turn. The advantages of the scheme set out here will be modest if the fourth type of policy process has no empirical-world referents. If the fourth cell is virtually devoid of entries, then the only gain over Lowi's categorization would derive from the way in which the questions underlying the paradigm facilitate the task of distinguishing between distributive, regulative, and protective/interactive policy processes.

It can, however, be shown that certain important foreign-policy acts are redistributive and do manifest a policy process which one predicts for the redistributive arena. One major class of such redistributive events is limited wars. Strictly speaking, not all limited wars are redistributive. The war may be so brief or otherwise limited that no opportunities would be foregone domestically. Those involved in actual fighting would be limited to professional military personnel—whose promotional opportunities are accelerated by a war (if they survive)—and the resources utilized would be drawn exclusively from those procured before the outset of the war. A limited war may at the outset widely be perceived as the initial phase of a general war[16] or as part of a general security strategy. *While these perceptions hold,* the policy process will be consensual-elitist rather than

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[14] *Politics, Pressures, and the Tariff* (New York: Prentice-Hall, 1935) and Lowi's summary of Schattschneider in "American Business . . . ," pp. 680–682.

[15] Harry Eckstein, ed., *Limited War* (New York: The Free Press, 1964); Lewis A. Coser, *The Functions of Social Conflict* (Glencoe: The Free Press, 1956).

[16] Kenneth N. Waltz, "Electoral Punishment and Foreign Policy Crises," in Rosenau, *Domestic Sources . . .*, p. 279, points out that in July, 1950 "just over half" the Americans polled thought the United States was "actually in World War III."

conflictual-elitist (Type III, rather than Type IV). Nevertheless, in practice limited wars have been searing events in American history. Knowing the history of the War of 1812, the War with Mexico, the Spanish-American War,[17] or the Korean War,[18] one would hardly be surprised to learn that the war in Vietnam has engendered high domestic conflict or to be informed that the politics of the war have at times resembled those of internal war. Viewed against a paradigm of policy processes—rather than, for instance, against a conception of foreign policy conducted by a rational, unitary decision maker—the nearly unlimited politics of limited war becomes readily understandable and in fact predictable. While the values at stake are less central to the core values of the society than those in genuine crisis situations, they extend far beyond those at issue in resource allocation questions. (Indeed, one ploy which the ruling group may adopt in order to mute conflict is to escalate the war to a point where general war may become a genuine possibility—in effect attempting to change the issue from a Type IV to a Type III policy process.) Limited wars are distinctively asymmetrical in their domestic political consequences. In discussing redistribution as a policy process relevant for certain internal events, Lowi observes, following Wallace Sayre, that the relevant cleavage is between those who provide the money and those who demand the services. What makes limited war even more acutely conflictual than most domestic redistributive issues is that the cleavage is not between money-providing and service-demanding segments of society but rather between those who demand the money and those who provide the services.

All four cells thus seem to have predictive implications for the analysis of the American foreign policy process. When an event is generally perceived as being distributive we can state with some confidence on the basis of previous case studies how the policy process is going to operate and which kinds of literature about politics are most likely to be productive of insights. In the case of crisis decision making, similarly, the highly elitist pattern involving the legitimate decision maker and his coterie of advisers acting "without their institutions" characteristic of Type III protective interactive policy processes will be observed. An event like a limited war with a dramatically asymmetrical impact on society and involving intense (although not core) nontangible as well tangible values soon will engender class warfare, generational confrontation, acute sectional strife, or other highly conflictual politics.

Directions for Future Research

Even without the research urgently needed to test the range of applicability of this paradigm, we can at least address ourselves tentatively to the range of states for which the paradigm has intuitive relevance. We have already sketched certain limits. One obvious qualification is that the state must have achieved minimal social and political consensus. In the absence of such consensus, a direct threat to the existence of the state will not be perceived as a threat to core values. A second qualification concerns the relationship of the regime to society. For our purposes, modern totalitarianism may be conceived as an effort by the regime artificially to re-create the qualities of the absolute monarch's state by insulating itself from the forces operative in "normal" complex industrial states (as well as by rendering itself impermeable to outside influence). If the regime succeeds, all policy disputes will resemble in their *process* those associated with the protective-interactive arena (Type III).

Within these clear limits, the paradigm may prove of some predictive value. Indeed, the task of comparison may be well served if we hold issue area constant and depict the manner by which, within a given issue area, differences in political structure and political culture affect the general contours of the policy process. For most developed, pluralist states, the paradigm certainly seems plausible.

Recent work by Roland Pennock, for instance, while not germane to foreign policy, suggests that what we have called the distributive arena is a major feature of British politics. Pennock attacks the conventional notion that the pork-barrel, log-rolling politics typical of the distributive arena are characteristically American. "It is true," Pennock grants, "that what has come to be defined as the pork-barrel phenomenon does not exist in Great Britain." Nevertheless, he asserts,

> If we look at the use of public funds for the benefit of identifiable groups, farmers, old age pensioners, recipients of general public assistance, manufacturers, and the like, something bearing a distinct family resemblance quickly appears. . . . Legally

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[17] The literature on each is extensive. Recent capsule depictions of the internal politics of the three are to be found in Samuel E. Morison, Frederick Merk and Frank Freidel, *Dissent in Three American Wars* (Cambridge: Harvard University Press, 1970).

[18] John E. Mueller, "Trends in Popular Support for the Wars in Korea and Vietnam," *American Political Science Review*, 65, (June, 1971), 358–75 shows the striking similarity in mass attitudes to the wars in Korea and Vietnam. He concludes that popular support decreases logarithmically with the increase in American casualties.

established minimum wages for agricultural labor, tariffs, and other benefits to manufactures, constitute in a larger sense a "package deal" that is not sharply dissimilar from the American "pork barrel." It is implicit log rolling, or vote trading, and it apportions benefits to special groups, the cost of which is spread throughout the taxpaying or, as the case may be, consuming public.[19]

My own research has indicated that in Yugoslavia a procedure for disaggregating resources to politically relevant units at the expense of the general interest is well established. In the Yugoslav context, those which count politically are republic party organizations and, frequently, large republic-specific enterprises. Evidencing only minimal national integration, Yugoslavia is a country whose political elite is fearful that tangible resource allocation questions, via the catalyst of republic-level nationalism, will be transformed into a redistributive issue. Thus, Yugoslavia displays an unusual propensity to disaggregate resource allocation decisions. For students of the American political process, the result is predictable. Even though for Yugoslavia to compete internationally economic rationality would prescribe an intensive strategy, the political process ordains a strategy of putting a few eggs in several baskets. A railroad terminating in the Croatian city of Rijeka is followed by its counterpart through Serbia to Bar. Ship building and harbor modernization funds are parceled out accordingly, each republic with an Adriatic outlet being indulged. Even major foreign service appointments are parcelled out; insufficient "ticket-balancing" may result in the vetoing of an ambassadorial appointment, for instance, by a *republic* assembly.

A recent study by Donald C. Hellmann[20] illustrates the relevance of the regulatory arena to the explanation of Japanese foreign-policy behavior in negotiating a peace agreement with the Soviet Union. Hellmann discovers that in the course of negotiating a peace agreement with the Soviet Union, "neither a clear public sentiment nor a broadly based articulate opinion emerged in [in Japan]."[21] He further finds that business interests generally played a negligible role in the decisional process—notwithstanding the close interlinkage of business, party, and government in Japan, an interlinkage bearing a resemblance to C. Wright Mills's power-elite image (Type IV) of American decision making. With a single exception, Hellmann concludes, the process of negotiating the peace agreement with the Soviet Union was highly elitist, in exactly the sense predicted for Type III issues.

The one exception proves interesting. "One economic issue, Japan's fishing rights in the northern seas, did figure significantly in Soviet relations. Accordingly, the Japanese fishing industry actively participated in the negotiations, seeking access primarily as an interest group working for its own benefit."[22] The fishing rights issue was tangible and highly asymmetrical in its domestic impact in Japan—a fact of which Moscow was well aware. Moscow in effect utilized the fishing rights facet of the settlement in order to alter the Japanese political process and thus to alter the outcome of the negotiations. By linking the peace settlement with fishing rights in the northern seas, what was otherwise a Type III protective-interactive issue became instead a kind of international licensing issue, i.e., a Type II regulatory issue.

One imagines, further, that the hypotheses pertaining to the arena of redistribution are transferable to other pluralist systems. A close comparison, for instance, of the domestic politics of Great Britain's Boer War and France's war in Indo-China with the domestic politics of the United States' war in Indo-China would presumably prove these events sufficiently congruent as to justify extending the projections about the redistributive politics of limited wars to all pluralist systems.

Finally, with regard to the protective-interactive arena it seems reasonable to assume tentatively that it is the United States which constitutes the limiting case. As such, we may assume that in other pluralist systems (several of which have less participant political cultures)[23] the decisional process will be no less elitist than in the United States. Bluntly put, crisis decision making appears to be much the same the world around; military grand strategy is likewise formulated in a manner according with a rational purposive model.[24]

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[19] J. Roland Pennock, "The 'Pork Barrel' and Majority Rule: A Note," *Journal of Politics,* 33 (August, 1970), 709–16, at 714.

[20] Hellmann, *Japanese Foreign Policy and Domestic Politics: The Peace Agreement with the Soviet Union* (Berkeley: Univ. of California Press, 1969).

[21] *Ibid.,* p. 149.

[22] *Ibid.,* p. 130. Hellmann's conclusion warrants comparison with another important case study in which fish played an important role. I have in mind, of course, Bernard C. Cohen's *The Political Process and Foreign Policy: The Making of the Japanese Peace Settlement* (Princeton: Princeton University Press, 1957). Once again the fish issue was the only really controversial aspect of the settlement. The peace settlement itself provoked little debate. The fisheries issue was settled separately by the North Pacific Fisheries Convention—a process which had all the attributes of regulatory politics. There were, thus, for the United States two issue areas: one, the peace settlement (Type III); the other, the regulatory fisheries convention (Type II). (For a similar assessment, see Rosenau, ed., *Domestic Sources,* pp. 48–49.)

[23] Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1965).

Perhaps the most intriguing test of the utility of the paradigm both for its predictive capacity and for its value in reorienting research, however, is not whether the paradigm has relevance for pluralist systems other than the United States. Rather, the crucial question stems from the implication of the paradigm that for comparable events in divergent political systems the policy process within an issue area is sufficiently similar as to explain or predict the foreign policy process. If such is the case, then, the paradigm has predictive value for states which are *neither* totalitarian *nor* pluralist but rather modern authoritarian systems characterized by a polyarchy of elites.

Such a country is the Soviet Union. Do the political structure and political culture of the Soviet Union so greatly shape the nature of policy formation as to preclude the value of issue-related hypotheses in the Soviet case? Nothing approaching a definitive answer can be given in this article—which is intended to prefigure my own further research and to redirect others' research orientation. We can state with considerable confidence that crisis decision making in the Soviet Union is highly elitist in the Type III protective-interactive sense.[25] More generally, we can assume that, in terms of process, events subsumed within the protective-interactive arena will correspond to that anticipated by the paradigm. The problem of course is that such a finding by itself is completely consonant with an explanation of the absence of domestic societal influences (save those transmitted through the personalities of the leadership) based on the nature of the political system.

We must look to the other arenas of power to speak to the relation of issues on policy process in the Soviet Union. Since for U.S. foreign policy a major manifestation of distributive arena politics is the intraservice parceling-out of the military budget, a replication of Samuel Huntington's *The Common Defense* in the Soviet context might be in order.[26] Such a replication would make it possible to ascertain whether there are fundamental distinctions in process, as in the United States, between Soviet force-procurement and Soviet strategic decisions, and whether the relevant service branches, ministries, and industrial enterprises are indulged through a disaggregation process brought about by "unprincipled" coalitions from within the Soviet counterpart to the military industrial complex.[27]

In the Soviet Union, during the post-Stalinist period, influence relations and communications patterns are asymmetrical. Given the interaction between the Soviet society and the regime, and given the redistributive character of limited wars, I would expect that, were the Soviet Union to become extensively embroiled in such a war (say in the Middle East), the domestic political process would resemble the American experience with the Vietman war; the only major qualification would be that the active role of large social aggregations (nationalities, generations of "fathers and sons" in conflict) would be more circumscribed because of Marxist-Leninist political culture and the Soviet political system. I would fully expect that elites and incipient counterelites would employ symbols designed to appeal to such large social aggregates and that the anticipated response of these political actors would be predominant in shaping the process of intra-elite political behavior. To take a case in point: had the Czechoslovak regime elected in 1968 to resist forcibly the Soviet invasion, and had it succeeded in transforming the Soviet action into a limited war, the fissiparous tendencies within the Soviet Union would have been immense. Indeed, if Alexander Dubcek had made credible the possibility Czechoslovakia would resist, the Soviet policy debate prior to the invasion would have been dramatically altered. The choice would no longer have been intervention with its foreign policy consequences or nonintervention with its foreign-policy costs and the dangers of domestic contamination (especially in the Ukraine)[28] from the Czechoslovak disease. Instead, the Politburo would have had to choose between nonintervention, with its possible domestic dangers, and intervention, with the attendant domestic costs of waging a limited war against a small, friendly, socialist state.

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[24] Samuel P. Huntington, *The Common Defense* (New York: Columbia Univ. Press, 1961).

[25] For evidence that Soviet decision making in the 1962 Cuban missile crises paralleled that in the United States, see U.S. Senate Committee on Government Operations, Sub-Committee on National Security Staffing and Operations, *Staffing Procedures and Problems in the Soviet Union 88th Congress, 1st Session,* 1963.

[26] More recent general studies of Soviet civil-miliary relations include: Roman Kolkowicz, *The Soviet Military and The Communist Party* (Princeton: Princeton Univ. Press, 1967); Thomas W. Wolfe, *Soviet Strategy at the Cross Roads* (Cambridge: Harvard Univ. Press, 1964); Arnold Horelick and Myron Rush, *Strategic Power and Soviet Foreign Policy* (Chicago: Univ. of Chicago Press, 1966).

[27] See, especially, "The Military-Industrial Complex: USSR/USA" *Journal of International Affairs,* 26 (Spring 1972), 1–97.

[28] Grey Hodnett and Peter J. Potichny; *The Ukraine and The Czechoslovak Crisis* (Canberra: Australian National Univ., Department of Political Science, Occasional Paper No. 6, 1970).

Where political structure probably does have a major impact on the Soviet foreign-policy process is in the arena of regulation. In recent years, considerable work has been done on Soviet interest groups,[29] much of it rather desultory in character and almost none of it bearing on foreign policy. That there is an interest articulation process seems no longer seriously at issue; what remains to be clarified is whether, given the prohibition against associational interest groups, the process is suitably depicted by conceptions derived from Western group theorists; i.e., whether the claims for resource allocation priorities are best conceptualized as the articulation of groups, groupings, or tendencies.[30]

In any event, viewed against an issue area framework, the traditional dichotomization of authoritarian and pluralist foreign-policy processes needs re-examination. To the extent that the argument developed herein withstands extensive testing against the empirical record, the impact of political structure on process would seem to be considerably more circumscribed than generally assumed. The ultimate conclusion may even be that differences in policy process across issue areas within a given state, the United States or the Soviet Union as cases in point, may be as great as differences in foreign-policy process within a particular arena of power for each.[31]

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[29] The best is Milton Lodge, *Soviet Elite Attitudes Since Stalin* (Columbus, Ohio: Charles E. Merrill, 1969), See, too, Joel J. Schwartz and William R. Keech, "Group Influence and the Policy Process in the Soviet Union," *American Political Science Review*, 62 (September, 1968), 840–851, and H. Gordon Skilling and Franklyn Griffiths, eds., *Interest Groups in Soviet Politics* (Princeton: Princeton Univ. Press, 1971).

[30] E.g., the articles in Skilling and Griffiths.

[31] After this article was written, Thomas L. Brewer published an important contribution to the literature linking issue-area and foreign-policy behavior in the American case. See his "Issue and Context Variations in Foreign Policy: Effects on American Elite Behavior," *Journal of Conflict Resolution*, 17 (March, 1973), 89–114.

Party and Incumbency in Postwar Senate Elections: Trends, Patterns, and Models*

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In the last half-century or so, incumbents of all offices for which we have data have exhibited greater strength at the polls than have their electoral rivals. Of those incumbents who have sought to retain their position, city councilmen have succeeded about 80 per cent of the time;[1] state legislators have defeated between 70 and 90 per cent of their challengers;[2] United States senators have achieved re-election in more than 80 per cent of their attempts (as shown in Table 2); and congressmen have emerged victorious at a phenomenal 90 per cent rate.[3] Pity the poor vulnerable governor: the last three decades have seen him taste victory "only" 64.7 per cent of the time.[4] It certainly appears that for the office-holder in an election, as for the property-holder in law, possession is worth about nine points. Yet it is not clear that incumbents win only, or even mainly, because they are incumbents. Other factors, such as the strength of the incumbent's party in the constituency involved, could be producing these results.

This paper is concerned primarily with incumbents for the office of United States senator. The two questions it seeks to answer are the following: (1) To what extent do incumbent senators win because they are incumbents, and to what extent do they win because their party is strong in their state? (2) How has the relative importance of party and incumbency in Senate elections changed during the postwar period? These questions merit investigation for at least three reasons. First, an understanding of the role of party in influencing the way Americans vote for subpresidential offices is a necessary complement to the knowledge thus far gained about how the American voter thinks and behaves in presidential contests. Second, the degree to which incumbency helps assure re-election speaks directly to some fundamental notions of the representativeness and (especially) the responsiveness of elected officials and institutions in the American political system. Third, the role of seniority in shaping the behavior of senators, and thereby the policies issuing from the upper chamber, is surely of sufficient consequence to warrant examination of the factors determining seniority itself, namely, re-election. In addition, the answer to the longitudinal question provides fuel for the ongoing debate about whether there is presently underway some sort of "realignment" of American electoral politics. We shall return to some of these questions at the conclusion of the paper.

The study consists of four main parts. First comes a brief review of some of the literature on congressional elections dealing with our central question—the relative influence of party and incumbency on electoral outcomes. The second part then describes some of the contours of phenomena probably unfamiliar to most scholars, namely, the extent to which senators have run for re-election during the period 1948–1970, and the extent to which those running have been successful. Both rates (running and winning) are strikingly high. The third part begins to deal with the problem of separating out the effects of party and incumbency in Senate elections. The fourth and final section constitutes the core of the analysis. It develops a theory accounting for the outcomes of Senate elections in terms of the major potential sources of electoral support given to the candidates. The theory is then represented symbolically in formal models for which multiple regression techniques are used to generate quantitative estimates of the relative importance of party and incumbency in all postwar Senate contests. This section closes with a brief summary of the findings and a statement regarding their implications for the changing shape of contemporary American politics.

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I am indebted to many people for their help in this study. Among those who improved this paper by reading and commenting on earlier drafts are Walter Dean Burnham of the Massachusetts Institute of Technology, Lawrence S. Mayer of the Virginia Polytechnic Institute and State University, Eric Uslaner of the University of Florida, and Ronald E. Weber of Indiana University. The two anonymous referees saved me from several errors and helped clarify the exposition. I am grateful to Washington University for making available the requisite computer facilities. Most of all, though, my thanks go to John D. Sprague of Washington University for sacrificing much time and energy to preach what he practices, superior scholarship. Finally, I gleefully adopt what may turn out to be the new fashion in academic writing: I claim full credit for whatever is worthwhile in the study, and blame all errors on my benefactors.

[1] At least this is the case in the San Francisco Bay area. See Kenneth Prewitt, "Political Ambitions, Volunteerism, and Electoral Accountability," *American Political Science Review*, 64 (March, 1970), 5–17, at p. 9.

[2] For example, in Connecticut during the period 1946–1958, 982 out of 1201 incumbents won re-election to the state legislature, for a success rate of 81.8 per cent (James David Barber, *The Lawmakers* [New Haven: Yale University Press, 1965], on p. 8); and David Leuthold reports an incumbent success rate of 73 per cent in Missouri state races during the period 1960–1964 (*Electioneering in a Democracy* [New York: Wiley, 1968], p. 127). See also Charles S. Hyneman, "Tenure and Turnover of Legislative Personnel," *Annals of the American Academy of Political and Social Science*, 195 (1938), 21–31, for data covering ten state legislatures over the decade 1925–1935. Finally, Wisconsin data collected by three of my students, Gary Londergan, Patrick Maguire, and Timothy Smock, show an even higher state-level success rate than those found elsewhere. Of the 50 state senators running for re-election in the period 1958–64, 46, or 92.0 per cent, won. Of the 315 state assemblymen running for re-election in the period 1956–62, 271, or 86.0 per cent, won.

[3] Computed from figures supplied in Charles O. Jones, *Every Second Year* (Washington, D.C.: The Brookings Institution, 1967), p. 68.

[4] Computed from figures supplied in J. Stephen Turrett, "The Vulnerability of American Governors, 1900–1969," *Midwest Journal of Political Science*, 15 (February, 1971), 108–32, at p. 118.

Literature Review: Incumbency and Congressional Elections

The major recent work on the electoral effects of House incumbency has been carried out by Milton C. Cummings, Charles O. Jones, and Robert S. Erikson. Cummings devoted an entire chapter to the topic in his excellent volume, *Congressmen and the Electorate,*[5] analyzing its relationship to such phenomena as "Presidential tide," third parties, and special local factors. One of Cummings's research questions was quite similar to what we ask of senatorial races: How important is the strength of the *party* in a given constituency, and how important is the strength of the individual (e.g., incumbent) *candidate* in shaping the electoral outcome? The answer his data convey is that "party" is by far the more consequential of the two, although incumbency does in fact have a significant independent impact.[6] For example, in the period covered by his analysis, incumbent representatives of the party that lost the presidency fared much better than nonincumbents of that losing party. More precisely, 84.7 per cent of the House candidates who won in the face of an unfavorable "Presidential tide" were incumbents.[7] Indeed, even in the party "rising with the tide," incumbents do better than nonincumbents, although the difference is of course much less pronounced.

Charles Jones's research in this area further underscores not only the "independent" electoral advantage of House incumbents, but also the importance of "party" in influencing election results. For example, even when an incumbent is not running (i.e., has died, retired, or been defeated in a primary), the *incumbent party* candidate is likely to prevail three-fourths of the time.[8] Obviously, then, there is more at work than "incumbency" in assuring a congressman re-election; the nature of the district is often such that his party is strong, or the opposition party is weak, or both.

Another effort to gauge the independent influence of incumbency in House elections is that of Robert Erikson.[9] He used multiple regression techniques with data from 1954 to 1960 to determine the percentage of the vote accounted for by incumbency. His analysis shows the advantage of House incumbency to be about two percentage points. In a subsequent study Erikson found that by the late 1960s, the advantage had grown. For example, in 1966 being a House incumbent was worth about five percentage points.[10] These patterns are especially suggestive in the light of the finding to be reported in the present paper that the advantage of *senatorial* incumbency, despite variation, has generally increased in the postwar period.

The principal works dealing directly with the electoral impact of senatorial incumbency are Donald R. Matthews's classic study, *U. S. Senators and Their World,*[11] and Barbara Hinckley's recent article, "Incumbency and the Presidential Vote in Senate Elections: Defining Parameters of Sub-presidential Voting."[12] Unfortunately, not all of Matthews's findings are directly comparable to those reported here since, for example, this study does not analyze senators in terms of his "career type" classification (i.e., Amateur, Professional, Patrician, Agitator). It is worth noting, however, that he was able to demonstrate clearly that a senator's chances for re-election are indeed related to the degree of party competition in his state.

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[5] Milton C. Cummings, Jr., *Congressmen and the Electorate* (New York: The Free Press, 1966), chapter 3.

[6] A fair inference, it would seem, from the data presented in Cummings, pp. 78–79.

[7] Computed from figures supplied in Cummings, p. 60.

[8] Charles O. Jones, "The Role of the Campaign in Congressional Politics," in *The Electoral Process,* ed. M. Kent Jennings and L. Harmon Zeigler (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966), pp. 21–41.

[9] Robert S. Erikson, "The Advantage of Incumbency in Congressional Elections," *Polity* (Spring, 1971), 395–405.

[10] Robert S. Erikson, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," *American Political Science Review,* 66 (December, 1972,) 1234–45, at pp. 1240 ff.

[11] Donald R. Matthews, *U. S. Senators and Their World* (New York: Vintage Books, n.d. First published 1960). See especially pp. 239–42.

[12] Barbara Hinckley, "Incumbency and the Presidential Vote in Senate Elections," *American Political Science Review,* 64 (September, 1970), 836–42.

Hinckley's article explores the question of "how much of the fluctuation in vote for Senator is explainable by the 'pull' of the presidential vote,"[13] on the one hand, and how much can be explained by senatorial incumbency, on the other. Employing as the standard the state-by-state estimates of base party identification developed by Ronald Weber and Frank Munger,[14] she tries to determine how much of the deviation from this standard can be statistically explained by her two variables. Through a simple correlation analysis, she concludes that in the period 1956–1966, 64 per cent of the fluctuation in voting is accounted for by this pair of nonidiosyncratic factors.

Although there is some overlap between Hinckley's concerns and those of the present paper, the two studies differ in several crucial respects. For one, while Hinckley's investigation utilizes data only from the decade 1956–1966, the present inquiry covers nearly the entire postwar period. More important, however, is the manner in which the two sets of data are analyzed. For example. Hinckley treats the entire decade as a single, highly aggregated unit of time, examining neither differences between the parties nor changes over time. The present work examines both. In addition, the statistical technique used in the Hinckley piece is correlation, while the principal method used here is multiple regression. This latter method seems clearly preferable for at least three important methodological reasons: (1) it forces us to be explicit about the *form* of the relationships we posit when we construct the models for which the parameter estimates are to be generated; (2) it enables us to concentrate primarily on the *shape* of the relationships being examined rather than focusing exclusively on the less interesting and really less enlightening "goodness-of-fit" criterion of correlation analysis; and (3) it provides us with actual *prediction equations* which we can then use to identify deviant cases, a procedure which has as one of its desirable outcomes the likelihood of learning something new and unexpected about the political world.[15]

Incumbency and Senate Elections: Postwar Patterns and Trends

This section addresses two principal sets of questions. To what extent have senatorial incumbents run for re-election? To what extent have those running been successful? In dealing with the first phenomenon, I shall deal exclusively with the overall pattern, investigating differences between presidential and off-year elections, and attempting to determine whether there is a trend over time. With the main concern of the paper, the "success rate" of senators, the analysis proceeds a step further. In addition to looking at the *overall* pattern, presidential and off-year differences, and trend, the analysis also separates *by party* and goes through the same procedure once more.

"Few die and none resign,"[16] says the only slightly overstated Capitol Hill aphorism describing the extent to which congressmen cling to their jobs. There is ample justification for applying it as well to the behavior of senators during the last quarter-century. Of the 392 general Senate elections during that period, 320 involved incumbents.[17] That is, 81.6 per cent of

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[13] Hinckley, p. 838.

[14] Ronald E. Weber and Frank Munger, "Party Identification and the Classification of State Party Systems," paper delivered at the 1968 Annual Meeting of the American Political Science Association, Washington, D.C. Their basic approach is to use what is known about the relationship between party identification and the demographic and socioeconomic characteristics of voters to construct a number of "voter-types." Determining the proportion of a state's electorate fitting each such "type" permits an estimate of each party's identifiers in that state. The approach is similar to that employed in the Simulatics project reported by Ithiel de Sola Pool, Robert P. Abelson, and Samuel Popkin in *Candidates, Issues, and Strategies*, rev. ed. (Cambridge: MIT Press, 1965).

[15] Arguments and evidence for this general position can be found, among other places, in Ronald J. Wonnacott and Thomas H. Wonnacott, *Econometrics* (New York: John Wiley & Sons, Inc., 1970), especially pp. 124–25; Edward R. Tufte, "Improving Data Analysis in Political Science," in *The Quantitative Analysis of Social Problems*, ed. Edward R. Tufte (Reading, Mass.: Addison-Wesley, 1970); and, for the *coup de grâce* to the goodness-of-fit criterion in theory construction, William Feller, *An Introduction to Probability Theory and Its Applications*, Volume II (New York: John Wiley & Sons, Inc., 1966), p. 52. The relevant portions of Feller's point are summarized and quoted in Adam Przeworski and Glaucio A. D. Soares, "Theories in Search of a Curve," *American Political Science Review, 65* (March, 1971), 51–68, in fn. 34 on p. 67.

[16] Clem Miller, *Member of the House* (New York: Charles Scribner's Sons, 1962), quoted at p. 93.

[17] *Incumbent* is defined here, and throughout the study, as a senator who has achieved the office through *election*, even a special election. Rates of running and winning always refer only to general election contests. A person holding the office at the time of the general election by virtue of appointment was not considered an incumbent. The reason for this distinction is presented in the final section.

Although I do not deal with primary contests here, I those contests saw a senator seeking to remain in office. While even this figure does not match the ninety-plus House percentage (for 1956–1966),[18] it is impressively high nonetheless.

As Table 1 indicates, there appears to be very little difference between presidential and off-years in the rate at which incumbents choose to seek re-election. The rate for presidential years is 82.1 per cent, for off-years 81.1 per cent. For purposes of comparison, it is worth noting that a similarly minor difference between the two types of election years occurred for the House during the decade 1956–1966: in presidential years—91.3 per cent of the races involved incumbents; in off-years—90.0 per cent.[19]

Figure 1 depicts the per cent of Senate contests involving an incumbent in each election year of the period under scrutiny. To determine whether these data contain a trend, the percentage of races involving incumbents (the "y-values") are regressed on time (the "x-values"), where $T = 1$ in 1948 and $T = 12$ in 1970. This results in the equation

$$\hat{Y} = 0.05\,T + 81.3, \tag{1}$$

showing the trend (slope) to be nearly zero. This technical operation adds weight to an impressionistic conclusion that there has been no real increase in the postwar period in the rate at which senators have run for re-election.

The belief prevalent on Capitol Hill is not only that congressmen *want* to remain in

Table 1. Number and Percentage of Senatorial Contests Involving Elected Incumbents: Presidential and Off-Year Elections Compared: 1948–1970

Presidential Election Years

Year	Number of Contests	Number Involving Incumbents	Percentage Involving Incumbents
1948	32	23	71.9
1952	32	27	84.4
1956	32	29	90.6
1960	33	28	84.8
1964	33	30	90.9
1968	34	24	70.6
Totals	196	161	82.1

Off-Year Elections

Year	Number of Contests	Number Involving Incumbents	Percentage Involving Incumbents
1950	32	25	78.1
1954	32	26	81.2
1958	32[a]	26	81.2
1962	34	30	88.2
1966	33	26	78.8
1970	33	26	78.8
Totals	196	159	81.1

Source: Congressional Quarterly, *Politics in America*, third edition (Washington, D. C.: Congressional Quarterly Service, 1969), pp. 133–144; *Biographical Directory of the American Congress: 1774–1961* (Washington, D. C.: Government Printing Office, 1961); *Congressional Directory*. 88th through 92nd Congresses (Washington, D. C.: Government Printing Office, various dates). Where discrepancies occurred between the government and the private source, the former was preferred.

[a] Two Alaska and two Hawaii contests omitted since they could not have involved incumbents.

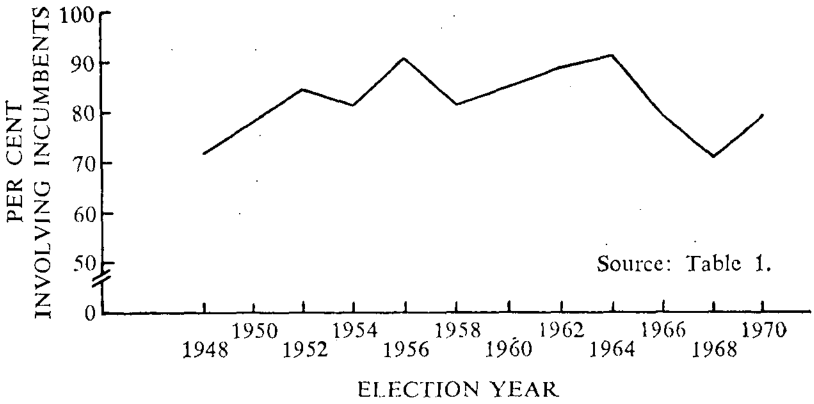

Figure 1. Percentage of Senatorial Contests Involving Elected Incumbents: 1948–1970

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might note for the record that only about 7 per cent of the postwar senators who sought renomination were denied it, a higher figure than the House's 1.6 per cent (from data in footnote 3), to be sure, but still small enough to pose no real problem to the present analysis.

[18] Computed from figures in Jones, *Every Second Year,* p. 68.

[19] Jones, p. 68.

Table 2. Number of Elected Senatorial Incumbents Running for Re-election; Number and Percentage of Winners: 1948–1970

Year	Number of Incumbents	Number Who Won	Percentage Who Won	Year	Number of Incumbents	Number Who Won	Percentage Who Won
1948	23	15	65.2	1960	28	27	96.4
1950	25	20	80.0	1962	30	27	90.0
1952	27	20	74.1	1964	30	28	93.3
1954	26	22	84.6	1966	26	25	96.2
1956	29	25	86.2	1968	24	20	83.3
1958	26	17	65.4	1970	26	23	88.5
				Totals	320	269	84.1

Source: See Table 1.

office, but also that they ought to be able to succeed in doing so. Charles Clapp, in *The Congressman,* summarizes the views of representatives thus: "There is a tendency to believe that, aside from isolated instances where an overriding issue is present, there is little excuse for defeat."[20] To what extent is this belief borne out in fact for the House counterparts, the Senators?

The number and per cent of senatorial incumbents running and winning are given in Table 2. Despite some fluctuations over time, the unmistakable generalization emerges that senators are usually returned to office. Out of 320 incumbents seeking re-election, 269, or 84.1 per cent, won. Moreover, there is virtually no difference between presidential and off-years. In presidential years, 135 out of 161 senators defeated their challengers, for a success rate of 83.8 per cent; in off-years, 134 out of 159 incumbents prevailed, for a rate of 84.3 per cent. Once again the pattern is similar to that for House members (for 1956–1966)[21]: presidential years—92.7 per cent were successful; off-years—92.3 per cent were successful.

A more detailed and direct comparison of the overall Senate rate with that of the House is provided by Table 3, showing the number and per cent of congressional and senatorial incumbents successful in their bids for re-election during the seven election years from 1954 to 1966. Both rates are high and unexpectedly close, at least given the conventional wisdom that senators are significantly more vulnerable: House—92.8 per cent; Senate—88.2 per cent.

Interestingly enough, the postwar trend has been toward even greater safety for senatorial incumbents, as Figure 2 amply illustrates. Re-

Table 3. Number of Congressional and Senatorial Incumbents Running for Re-election; Number and Percentage of Winners: 1954–1966

Year	Number of House Incumbents	Number Who Won	Percentage Who Won	Number of Senatorial Incumbents	Number Who Won	Percentage Who Won
1954	401	379	94.5	26	22	84.6
1956	405	389	96.0	29	25	86.2
1958	391	354	90.5	26	17	65.4
1960	398	372	93.5	28	27	96.4
1962	382	368	96.3	30	27	90.0
1964	389	345	88.7	30	28	93.3
1966	402	362	90.0	26	25	96.2
Totals	2,768	2,569	92.8	169	149	88.2

Source: House figures computed from data supplied in Charles O. Jones, *Every Second Year* (Washington, D.C.: The Brookings Institution, 1967), p. 68, and, for 1954, in Charles O. Jones, "The Role of the Campaign in Congressional Politics," in *The Electoral Process*, ed. M. Kent Jennings and L. Harmon Zeigler (Englewood Cliffs, N.J.: Prentice-Hall, 1966), at p. 24. Senate figures, see Table 1.

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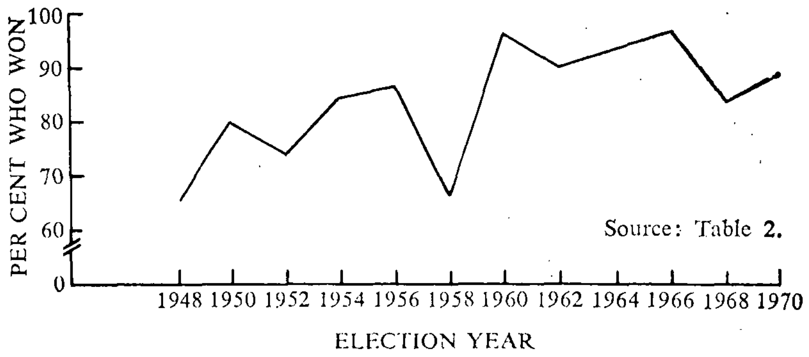
[20] Charles Clapp, *The Congressman* (Garden City, N.Y.: Anchor Books, 1963), p. 374.

[21] Jones, *Every Second Year,* p. 68.



Source: Table 2.

Figure 2. Percentage of Elected Senatorial Incumbents Running for Re-election Who Won: 1948–1970

gressing the percentage of successful senators on the time variable produces the equation

$$\hat{Y} = 1.68\,T + 73.0, \tag{2}$$

where T = 1 in 1948. In other words, each election year sitting senators have become, on the average, almost "two per cent safer." Over a period of twelve elections, such an average increase becomes substantial, indeed.

This increase in safety for incumbents is, of course, not confined to the upper house. Some indication of the trend for the lower chamber is contained in the observation of Charles Jones that, for the Congress, ". . . between 1932 and 1940, 69.9 per cent of the districts were won by the same party in all five elections; between 1942 and 1950, 74.0 per cent were won by the same party; and between 1952 and 1960, 78.2 per cent were won by the same party."[22]

Useful as these overall findings are, they may very well obscure some important differences between the parties. As the comparable data arrayed in Table 4 effectively demonstrate, that is, in fact, the case. While about 90 per cent of Democratic senators won re-election, "only" about three-fourths of the Republicans were so fortunate. Whether this difference is due to the Democrats' superior campaigning skill, greater money and charm, and more attractive families, or whether this difference in *incumbency* success rate is due more systematically to dif-

Table 4. Number of Elected Senatorial Incumbents Running for Re-election; Number and Percentage of Winners, By Party: 1948–1970

Year	Democrats			Republicans		
	Number of Incumbents	Number Who Won	Percentage Who Won	Number of Incumbents	Number Who Won	Percentage Who Won
1948	10	10	100.0	13	5	38.5
1950	15	11	73.3	10	9	90.0
1952	9	6	66.7	18	14	77.8
1954	17	16	94.1	9	6	66.7
1956	13	12	92.3	16	13	81.2
1958	12	12	100.0	14	5	35.7
1960	18	17	94.4	10	10	100.0
1962	18	17	94.4	12	10	83.3
1964	23	23	100.0	7	5	71.4
1966	14	13	92.9	12	12	100.0
1968	17	13	76.5	7	7	100.0
1970	20	18	90.0	6	5	83.3
Totals	186	168	90.3	134	101	75.4

Source: See Table 1.

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[22] Jones, p. 68. Note, however, that this trend refers to the safety of districts for the *party*. Although we assume the general pattern is similar for the safety of *incumbents*, an empirical demonstration of that assumption's validity is clearly beyond the scope of the present paper.

ferences in *party* strength in the electorate, remains to be seen. Indeed, it is precisely this question that is explored initially in the next part, and analyzed further in the final section of this paper.

Comparing the pattern for each party in presidential versus off-year elections reveals that there is no appreciable difference *within the parties* between the two types of election years. For the Democrats, the presidential year success rate is exactly 90.0 per cent, with the off-year rate an almost identical 90.6 per cent. For the Republicans, the difference is only slightly greater. In presidential election years, 76.1 per cent win, while in the off-years the figure is 74.6 per cent.

To determine whether the postwar trends for the two parties differ, consult Figure 3, which presents in graphical form the percentage of each party's senatorial incumbents who were successful each election year. As with the earlier estimates of trend lines the percentage successful is regressed on time to produce a trend for the Democrats described by the equation

$$(3) \qquad \hat{Y} = 0.46\,T + 86.5,$$

where $T=1$ in 1948. Although there is some movement toward greater safety, it is fairly small. But according to the equation, the Democrats start out so safe (an average of about 87 per cent win in 1948, solving at $T=1$) that even the slight increase of five or six percentage points over the past quarter century could have important political consequences.

The trend for the Republicans is far stronger. Their equation reads

$$(4) \qquad \hat{Y} = 2.91\,T + 58.4,$$

where $T=1$ in 1948. Here we find that Republican senators won only about 60 per cent of their re-election bids immediately following the War, but now win, on the average, more than 90 per cent of them. This marked change suggests once more that we must ask whether the Republican gain is due to their party having grown stronger (and/or the Democrats weaker), or to a greater independent electoral importance assumed by incumbency.[23] Analysis of this question is, of course, the task of the next two sections of the paper, with the final part paying special attention to the longitudinal aspects of the problem.

The discussion up to now has been confined to nationally aggregated patterns. Within-state patterns are suggestive as well.[24] For example, it is interesting to note that California had the

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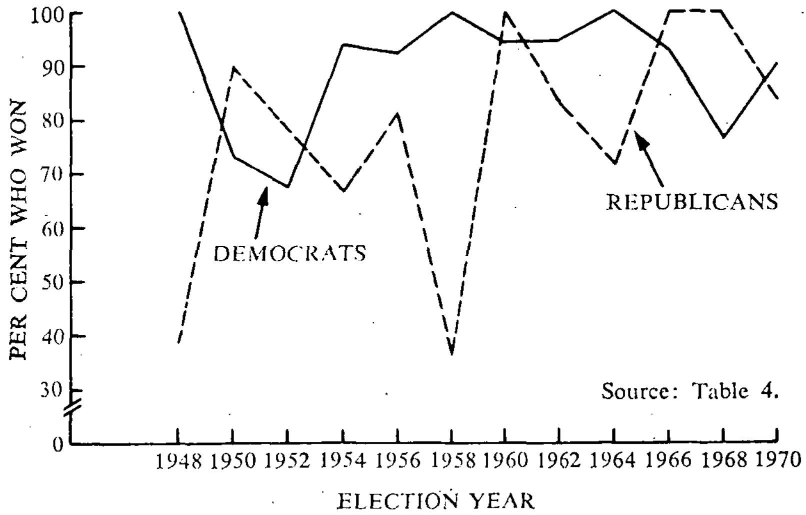
[23] Naturally, trend lines such as these are highly sensitive to extreme values at the end points of the time series. For example, adding 1946 to the present pair of series brings the two trend lines a good deal closer together due to the high success (100 per cent) of Republican incumbents and the low success (66.7 per cent) of Democrats in that year. The Democratic trend is then described by the equation, $\hat{Y} = 1.12\,T + 81.1$, while the Republican estimate becomes $\hat{Y} = 1.54\,T + 69.8$, where $T = O$ in 1946 in both instances. And yet despite such extreme readings, the difference between the parties does still remain, and still points to the same question: why the different rate of change for each party over time?

[24] These within-state patterns are based on data from 1946–1970.



Source: Table 4.

Figure 3. Percentage of Elected Senatorial Incumbents Running for Re-election Who Won: by Party: 1948–1970

smallest proportion of its postwar Senate contests involving an incumbent—four out of nine; the remaining states all had a sitting Senator vying for the seat in at least half, and sometimes all, of their Senate races. And a second observation, this one suggestive of the electoral power of incumbency, is that three of the four states that did worst in returning senators to office still re-elected incumbents fifty per cent of the time.[25] A success rate of one-half is what would be expected under the assumption of purely random outcomes of a win-lose situation, so the fact that the remaining forty-six states had success rates in excess of fifty per cent is consistent with the notion that incumbents have factors working systematically in their behalf.

Preliminary Analysis of the Relationship Between Party and Incumbency

Party strength is one of those systematic factors we suspect affects the incumbent success rate. Some idea of the extensive overlap, in a quite literal sense, between the two can be gained from the following simple "exercise": List all those states where both Senate seats were won by the same party in every general election of the last two-and-a-half decades. There are fourteen such states. Ninety-nine incumbent senators ran for re-election in those states and ninety-seven won. Now relax the criterion for selection of the states to include those where the same party won both seats every time but once. There is now a total of twenty-two states in the list. Next, make up a separate list of all those states in which a senator seeking to remain in office always succeeded in doing so. There are twenty-six such states. This exercise reveals that seventeen states appear on both lists, a considerable overlap.

If this overlap operates as hypothesized, then the more politically competitive states ought to have lower senatorial incumbency success rates, and vice versa. One way to examine this hypothesis is to group the states according to some measure of political competitiveness and to compare the success rates. The rationale underlying Table 5 is that the "regional" groupings are much less competitive than the "large industrial" states, and that states in neither category fall between these two extremes of competitiveness. The senatorial incumbency success rates follow the expected pattern.

A somewhat more sophisticated classification of the states according to party competitiveness has been developed by Austin Ranney.[26] His scheme averages together four measures of Democratic electoral strength in state offices over the period 1946–1963 to produce an "index of competitiveness" that ranges from 1.0000 (complete Democratic control) to 0.0000

Table 5. Number of Senatorial Incumbents Running for Re-election in Certain Categories of States; Number and Percentage of Winners: 1946–1970

Category of States[a]	Number of States	Number of Incumbents	Number Who Won	Percentage Who Won
Total South	11	77	76[b]	98.7
Deep South	(5)	(38)	(38)	(100.0)
Fringe South	(6)	(39)	(38)	(97.4)
"Solid North"	7	49	48[b]	98.0
Border	6	41	29	70.7
Large Industrial	7	44	31	70.5
Remainder	19	126	98	77.8

Source: See Table 1.

[a] The states are categorized as follows: Deep South: Alabama, Georgia, Louisiana, Mississippi, South Carolina; Fringe South: Arkansas, Florida, North Carolina, Tennessee, Texas, Virginia; Border: Kentucky, Maryland, Missouri, New Mexico, Oklahoma, West Virginia (Maryland's Senators won only twice out of six tries, thus accounting for the lower than expected success rate for this category of state. If Maryland were omitted from the calculations, the per cent for this group would be 77.1); Large Industrial: California, Illinois, Michigan, New Jersey, New York, Ohio, Pennsylvania; "Solid North": Kansas, Maine, Nebraska, New Hampshire, North Dakota, South Dakota, Vermont.

[b] For the curious, the one Southern Democrat failing re-election was Albert Gore of Tennessee in 1970, and the one "Solid North" Republican was Payne of Maine, who lost to Edmund Muskie in 1958.

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[25] Maryland retained in office only two of the six senators seeking reelection. The three states returning an even half of their incumbents are Kentucky, Pennsylvania, and Wyoming.

[26] Austin Ranney, "Parties in State Politics," in *Politics and the American States,* ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1965), pp. 61–99; see especially pp. 63–70.

Table 6. Number of Senatorial Incumbents Running and Winning in the States: (States Classified According to Degree of Inter-Party Competition[a]) Percentage of Winners, By Category: 1946–1970

One-Party Democratic			Modified One-Party Democratic			Modified One-Party Republican		
State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won
So. Carolina	7	7	Virginia	8	8	Wisconsin	8	7
Georgia	7	7	No. Carolina	6	6	N. Hampshire	8	8
Louisana	8	8	Tennessee	6	5	Iowa	6	4
Mississippi	9	9	Oklahoma	6	5	Kansas	5	5
Texas	6	6	Kentucky	6	3	Maine	7	6
Alabama	7	7	Arizona	7	6	So. Dakota	5	5
Arkansas	8	8	W. Virginia	9	7	No. Dakota	9	9
Florida	5	5	Maryland	6	2	Vermont	7	7
			New Mexico	7	7			
Totals	57	57		61	49		55	51
Per cent		100.0			80.3			92.7

Two-Party

State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won
Alaska	3	3	Colorado	6	5	Illinois	8	5
Missouri	7	5	Montana	7	6	Idaho	7	5
Rhode Island	7	7	Minnosota	6	4	Michigan	7	5
Washington	8	7	Utah	9	6	New Jersey	6	6
Delaware	8	5	Connecticut	6	4	Indiana	7	6
Nevada	8	7	Pennsylvania	8	4	Oregon	6	4
Massachusetts	8	6	California	4	3	Ohio	6	4
Hawaii	3	3	Nebraska	8	8	Wyoming	6	3
						New York	5	4
						Totals	164	125
						Per cent		76.2

Source: See Table 1.

[a] From Austin Ranney, "Parties in State Politics," in *Politics in the American States*, ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1965), pp. 61–100. The states are listed here within each category according to their decreasing "Democratic-ness." For a brief explanation of the "index of competitiveness" upon which these categories are based, see the text.

(complete Republican control). An index score of 0.5000 signifies that the parties are perfectly competitive. The resulting clusters of states suggested the categories reproduced here in Table 6.

Once again a clear relationship between party and incumbency emerges. The most competitive states have the lowest senatorial incumbency success rates, and the least competitive the highest. One interesting sidelight is the difference between the success rates in modified one-party Democratic states (80.3 per cent) and modified one-party Republican states (92.7 per cent). A possible explanation for the difference may lie in the extent to which the *state* party systems are allied to or coincide with the *national* party systems in Democratic-dominated versus Republican-dominated states, for while Ranney's measure is based exclusively on state offices, the position of senator is usually considered a national office.

Because both the Ranney index and the incumbency success rate can be considered interval measures, and because a value for both variables can be assigned to each state, performing an experiment may identify somewhat more precisely just how these two phenomena are related. Since the concern in this instance is more generally with "competitiveness" than specifically in Democratic or Republican strength, we first reconstruct part of the Ranney index. Using the measure as it is given for the interval 1.0000 to 0.5000, the interval

0.4999 to 0.0000 is recalculated by subtracting each value therein from 1.0000. This more general index of competitiveness then varies from 1.0000, non-competitive, to 0.5000, perfectly competitive.

The hypothesis being investigated is that the more politically competitive the parties are, the less likely to achieve re-election a senator will be. Hence, the dependent variable, the senatorial incumbency success rate (measured by the proportion of times sitting senators seeking re-election in a given state during the postwar period were successful), is regressed on the independent variable, the revised index of competitiveness. The relationship between these phenomena is then expressed by the equation

$$(5) \qquad \widehat{SR} = 0.58\,C + 0.42,$$

where $\widehat{SR}$ is the senatorial incumbency Success Rate and C is the degree of Competitiveness.

This result provides an extremely interesting substantive interpretation of the independent impact of senatorial incumbency on electoral outcomes. What it says is that in a completely noncompetitive state, whether controlled by Democrats or Republicans, senators will win re-election every time (solving for $C = 1.0$). At the other extreme, where the state parties are absolutely competitive ($C = 0.5$), senators will still defeat their challengers about seven times out of ten. If this result is to be believed, the "independent" (nonparty-related) advantage a senatorial incumbent brings to the polls is very real and quite substantial. The odds are clearly in his favor. Two caveats must of course be entered, however. One is that the result obtained employed what must be regarded as two fairly crude measures, despite the pseudo-accuracy of carrying out the estimates to four decimal places. The second is that the Ranney index, being based solely on state offices, would be expected to produce some "slack" in its relationship to the incumbency phenomena for a national office. In any event, however, the least that can be said of this finding is that it is highly intriguing and suggestive of even better methods of generating precise estimates of the types of relationships in which we are most interested.

Models of Party, Incumbency, and "National Tides"

Whatever the specific nature of the role of party and incumbency in Senate elections, there is obviously persuasive evidence that they both exert great influence on those electoral outcomes. To understand fully the electoral process therefore requires gauging as accurately as possible their separate and independent impact on such elections. The mode of analysis employed here involves representing the theory in a formal model for which are then generated quantitative estimates of the appropriate parameters through least squares multiple regression. In constructing the model the strategy is to draw upon available knowledge about political behavior in order to approximate "reality" as closely as possible. In short, the procedure involves embodying certain beliefs about the political world in a formal representation, and then testing those beliefs against what data are most relevant to the questions at hand.

Theory and Model. The phenomenon to be explained, or predicted, is the percentage of votes received by postwar senatorial candidates, given certain other information about the election. To accomplish this task involves identifying the major potential sources of a senatorial candidate's electoral support. The argument presented here is that there are three systematic, measurable sources common to all Senate races and one group of unsystematic, unmeasurable sources peculiar to each contest. "Party loyalty," incumbency, and "national tides" fall under the first rubric, while such idiosyncratic factors as personality, issues, and local conditions compose the second group.

Party identification exerts an extremely powerful influence on voting in the United States. Angus Campbell and his associates estimated that of those voters in their 1952 samples who had formed no evaluations of "political objects" (i.e., no intervening variables entering into their vote choice), 75 per cent voted their party identification.[27] Moreover, a substantial degree of straight-ticket voting is associated with party loyalty.[28] In states where straight-ticket voting is facilitated by the ballot form used, 77 per cent of the strong identifiers supported their party right down the line, with a full 70 per cent of the weak identifiers following suit. Even in states where such voting is hindered by the form of the ballot, 55 per cent of the weak identifiers chose to stick with one party, while just as many strong identifiers as in the "easy" states—77 per cent—selected the straight ticket. And equally significant is the nature of the split ticket, when that route is chosen. The finding is that when voters do cross party lines, they are far more likely to do so for state and local offices than for senator or congressman.[29]

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[27] Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: John Wiley and Sons, 1960), p. 142.

[28] The following figures are taken from Angus Campbell and Warren E. Miller, "The Motivational Basis of Straight and Split Ticket Voting," *American Political Science Review*, *51* (June, 1957), 293–312.

But it must be emphasized that there are in fact degrees of party identification, such as the "strong" and the "weak" mentioned above, and that there are demonstrable differences in the manner in which those degrees of attitudinal commitment are translated into "party votes," as the ballot-form example amply illustrates. The most loyal and deeply committed party identifiers will vote "party" under even the most severe cross-pressures and counter-trends, while the remainder will be far more likely to defect due to election-specific personalities and the issues of the day. It will be important to bear this in mind in selecting the indicator of "party loyalty," for one is needed which taps the true core of the party supporters.

The second potential source of a candidate's support is derived from the fact of being the incumbent. Above and beyond party influences, an incumbent is likely to receive an additional increment of votes because he holds the office at the time of the election. This extra margin is evident in the literature review of the first part of this paper, the descriptive materials of the second part, and the experiment with the revised Ranney index of competitiveness and the various cross-tabulation analyses of the immediately preceding section.

We should be explicit about the reason the current office-holder has an electoral advantage, for it should not be thought that "incumbency" is some sort of magical, mysterious phenomenon. There are good solid political explanations for the incumbent's garnering additional support. Although they have been examined often enough by scholars to permit our doing without a detailed exposition here, it may still be worthwhile to provide at least a skeletal framework of the larger arguments.[30]

(1) The incumbent is likely to have substantial political skill and experience, and often a superior political organization, owing to his having been through the mill, successfully, at least once previously.

(2) He should enjoy greater voter recognition than his opponent through having been in the public eye during the last campaign and during the preceding years of his term, through his use of the franking privilege, the carrying out of casework, appearances at public events, and so forth. This simple fact of public awareness can prove incredibly powerful. Stokes and Miller[31] have shown that in the 1958 congressional races, of those voters who were aware only of the candidate of their own party, 98 per cent voted for him; conversely, of those aware only of the other party's candidate, 40 per cent voted for him. Who, we may ask, is the average voter likely to be aware of more, an incumbent senator, or his challenger? While there are certainly some cases where the challenger is even more well-known than the senator (suppose Bart Starr ran against Senator Gaylord Nelson in Wisconsin, or Joe Namath ran for the Senate in New York . . .), the reverse is surely more common. We can probably say, then, that for senators as for congressmen, incumbents are more salient to the voters than are nonincumbents, and that "in the main, recognition carries a positive valence; to be perceived at all is to be perceived favorably."[32]

(3) The office-holder is likely to have a fair amount of political clout within his party because of patronage, fund-raising activities, prestige, favors of all kinds. This should make it easier for him to acquire a wide variety of campaign resources, from workers, to information, to credit.[33] Moreover, since fund-providers probably prefer to back a winner,[34] and since the success rate of incumbents has presumably not been lost on potential contributors, the incumbent should find money considerably easier to come by than his opponent.

In addition to party and incumbency, a third systematic influence on the electoral support received by a senatorial candidate is what I term "national tides." Although it is *not* generally true that "As Maine goes, so goes the nation," it is generally true that "As the nation goes, so goes your state."[35] The point is, of course, that state elections held at the same time as a national election tend to be influenced by the national issues being raised and discussed, as well as by the national candidates in a presidential election year. A number of scholars have worked with the notion of "presidential tides," but they tend to neglect the very real fact that there are often equally strong trends, or "tides," in the off-year as well, and that they usually register similarly in each state. Clearly, 1958, although an off-year election, exhibited such a "tide," and few are in doubt as to its direction.

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[29] Campbell and Miller; see their Table VI, at p. 306.

[30] A brief review of the electoral advantages of congressional incumbency can be found in Stephen K. Bailey, *Congress in the Seventies* (New York: St. Martin's Press, 1970), pp. 9–11. See also Robert Erikson, "The Advantage of Incumbency in Congressional Elections."

[31] Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," chapter 11 in *Elections and the Political Order,* by Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes (New York: John Wiley and Sons, 1966), pp. 194–211, at p. 205.

[32] Stokes and Miller, in Campbell et al., *Elections and the Political Order,* p. 205.

[33] See Leuthold, *Electioneering in a Democracy,* for an admirable discussion of congressional campaigns largely in terms of resource acquisition. His observations and findings undoubtedly apply, *mutatis mutandis,* to senatorial contests as well.

[34] Such an inference can easily be drawn from Leuthold, Table 1, p. 67, at least insofar as labor unions are concerned. In addition, chapter 6 deals with the whole range of fund-raising questions. A comment regarding incumbents is telling: "Nonincumbents may have needed more money but they found it harder to raise, and ended up with less money than incumbents" (p. 77).

Just as the third part of the paper established that party and incumbency are related both to each other and to Senate electoral outcomes, it can be demonstrated here that "national tides" and incumbency are similarly related. This demonstration will add some indirect evidence that the phenomenon of "national tides" is worth including in the analysis. Let us take as a rough indication of the overall "national tide" for a particular year the difference between the percentage of the total votes cast for a party's House candidates this election year and the percentage cast for their candidates the previous election year. For example, the 1958 Republican "tide" was −5.3 per cent, the 1958 Democratic "tide" was +5.3 per cent (the 1958 House percentage minus the 1956 House percentage). Next we take an analogous difference for each party's senatorial incumbency success rates in those two election years. Comparing the two differences for each election year of the postwar period reveals that in ten out of twelve elections the differences have moved in the same direction. The only two years they do not (and the discrepancy occurs in the same years for both parties) are 1952 and 1962, and in both instances the margin by which they fail to do so is almost negligible. Furthermore, the R^2 produced by a regression of the (differences between) senatorial incumbency success rates on the "national tides" variable exceeds 57 per cent for both parties.

So the conclusion is that "tides" do indeed systematically influence (Senate) elections. Yet the "tides" will have different effects in different states and, unlike the other phenomena, can have a punitive influence on the outcomes for one party while helping the other party. The most extreme example of the differential effects by state occurred in 1964. While virtually the entire country was sweeping toward Lyndon Johnson and the Democrats, the five states comprising the Deep South were moving quite dramatically in the opposite direction, against the Democratic "tide." Such an occurrence is of course perfectly consistent with the argument that these states were in fact responding to the same national issues and personalities as the remaining states; that they chose a different stand on the issues (especially race) and personalities is of no consequence to the theory. It does remind us, however, that since our analysis is state-centered, the "tides" variable must vary across states as well as over time. And it underscores the fact that failure to control for such phenomena in the analysis could have a confounding effect on our understanding of the relationship of central interest here, that between party and incumbency.

The final source of votes for a candidate involves such idiosyncratic elements as personality, issues, and local factors. In some elections, of course, these could conceivably outweigh all others, though it is the contention here that such an event is rare. The effect of these idiosyncratic factors is not measured directly, but is estimated indirectly, as will be shown below.

The reasoning thus far forms a basis for representing symbolically what has been stated in words. Bear in mind, however, that our "theory" comprises our words and sentences and hypotheses about politics; the model is but a shorthand formalization of its main features. It cannot be overemphasized that what we constantly seek are sentences about the political world; the symbols and numbers we employ are merely means to that end.

With that said, then, the theory is represented thus:

$$S = f(P, I, T, Id) \tag{6}$$

where:

S = the per cent of the votes received by a particular senatorial candidate,
f = means "a function of,"
P = "core party loyalty," or "base party vote,"
I = incumbency,
T = "national tides,"
Id = idiosyncratic factors.

In words, this says that the percentage of votes received by a particular senatorial candidate is a function of the proportion of the voters who are "core party loyalists"; his status as an incumbent or nonincumbent; the magnitude and direction of the "national tides"; and, finally, his personality, the issues raised, and other local factors (e.g., the strength of the party organization and his relationship to it, the power of the labor unions and their traditional role in elections in that state, factionalism, etc.).

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[35] The close relationship between national and state electoral results is documented in Louis Bean, *How to Predict Elections* (New York: Alfred A. Knopf, 1948) and in V. O. Key, Jr., *American State Politics* (New York: Alfred A. Knopf, 1956), especially in chapter 2.

The theory is represented symbolically in this most *general* form to emphasize that there are any number of different *specific* forms the relationships could take. These various sources could be seen as being strictly additive in their effects, as the formal model in (9), below, indicates. But it is also plausible to assert, for example, that the particular portion of the voting population that will be most likely to vote for a candidate because he is the incumbent are those who are not "core party loyalists" for either party, for those loyalists are so locked into their party that they will vote for or against a candidate purely on the basis of his party label. Then, if P_d represents the proportion of the electorate which supports the Democrats under any and all conditions, and P_r represents those solidly loyal to the Republicans, then $1 - (P_d + P_r)$ stands for the proportion in neither group. Given our working hypothesis, and still assuming for convenience that the effects of the phenomena in (6) are additive, this revised theory could be represented as

$$\begin{aligned} S_d = a(P_d) + b(I)[1 - (P_d + P_r)] \\ + c(T) + Id \end{aligned} \tag{7}$$

where the statement has been restricted to Democratic candidates for ease of comprehension. Naturally, *a, b,* and *c* are parameters measuring the increment of support given the candidates from the indicated source.

An alternative formulation involves the contextual argument that although a certain proportion of the population is likely to vote for one party because of some individual loyalty or commitment, there is also a group effect. Hence, as Warren Miller, for example, found, at the higher concentrations of party strength, the interaction among the party faithful increases, as does the probability of their voting for their party's candidates.[36] This might be represented by the model

$$\begin{aligned} S = a(P * P) + b(P) + c(I) \\ + d(T) + Id \end{aligned} \tag{8}$$

where $(P * P)$ is, under random mixing assumptions, the probability that a given interaction involves one party loyalist interacting with another party loyalist, and *a* is the probability that the interaction will result in an additional vote for the senatorial candidate of that party. The rest of the model stands as before.[37]

These highly abbreviated examples are intended to show that there are a large number of different *forms* of the relationships among the phenomena selected here as most important in explaining Senate elections.[38] The choice made below is not the only one available. Indeed, the model used is the simplest possible, for the hypothesis is that the effects of these phenomena are both linear and additive. Hence, the expression

$$S = a(P) + b(I) + c(T) + Id \tag{9}$$

is the model for which are estimated the parameters *a, b,* and *c,* and the intercept, *Id.*

Variables. The task now is to select the variables to be used as indicators of the phenomena the theory stipulates as most important. The variable for *S* is simply the percentage of the total popular votes cast for each major-party senatorial candidate in each state at each general election from 1948 to 1970.[39]

Several contenders could serve as the variable measuring *P*. Two are especially attractive. The first is the set of estimates of base party identification developed initially by Ronald Weber and Frank Munger and used by Barbara Hinckley in her article on Senate elections cited earlier. The measure is described briefly in footnote 14 of the present paper. Unfortunately, two serious limitations prevent their adoption here. One is that the estimates are currently available only for the period 1954–1966, yet the present study examines the entire postwar period. The other difficulty attending the Weber estimates is that they are

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[36] Warren E. Miller, "One Party Politics and the Voter," *American Political Science Review,* 50 (September, 1956), 707–25.

[37] For a stimulating analysis of left voting which emphasizes the importance of the *form* of the relationships among phenomena, as well as a more detailed development of the "contextual" argument, see Przeworski and Soares, "Theories in Search of a Curve." On the problem of forms of relationships as it relates to an American electoral situation, see Walter Dean Burnham and John D. Sprague, "Additive and Multiplicative Models of the Voting Universe: The Case of Pennsylvania, 1960–68," *American Political Science Review,* 64 (June, 1970), 471–90.

[38] The two alternative forms mentioned here, as well as several others, are developed further and the parameters estimated in my paper, "Form and Substance in Theory Construction: Electoral Behavior in Postwar Senate Elections," presented at the Annual Meeting of the American Political Science Association, Washington, D.C., September 5–9, 1972. In addition, the contextual argument is elaborated in that paper several steps beyond the sketch given here.

[39] The source relied upon for all election data was the *Statistical Abstract of the United States* (Washington, D.C.: Government Printing Office, various dates). Errors noted were corrected (e.g., on two occasions special Senate elections were listed in the general election category) but otherwise the figures were used as given. Incumbency information was culled from the sources listed in Table 1 of the present paper.

attitudinal measures, while a behavioral measure is preferred. There are important differences between base party *identification* and base party *vote.*

Consequently, the "base party vote" measure used for the variable *P* in the model is constructed in the following manner. (1) Compute the per cent of popular votes cast for the House candidates of each party in each state for each election year from 1946 to 1970. For example, for a given state in a given year, the Democratic figure is calculated thus:

$$\frac{\text{Total number of votes cast for all Democratic House candidates}}{\text{Total number of votes cast for all House candidates}}$$

(2) For a given election year, say, 1960, choose as the "base party vote" for each state (i.e., the value for *P*) the *lowest* per cent of votes for House candidates cast in that state during the five election years of which the "target year" (in this instance, 1960) constitutes the midpoint year. In other words, for a particular state (e.g., Wisconsin) look at the percentage of votes garnered by the congressional candidates of a particular party (e.g., the Democrats) in each of the following years: 1956, 1958, 1960, 1962, 1964. The lowest Democratic percentage of those five elections is the Democratic "base party vote" for the midpoint year of the series, 1960. The same operation is carried out separately for the Republicans.[40]

Observe that this procedure involves going a maximum of only four calendar years away from the target year to get the value of *P* for that year. Knowledge of the glacier-like pace at which *core* ("strong") party loyalty changes certainly suggests that such a short span of time is very unlikely to misrepresent the size of the group of loyalists. And yet there is being employed a behavioral measure of the proportion of the electorate in a given state which, by its *actions*, has demonstrated that it will support a given party under the severest conditions "of recent memory."

Two principal difficulties, neither of them crippling, make this choice of variable less than ideal. For one, although the vote for House candidates probably approaches better than most the notion of a "normal" party vote,[41] it is not perfect. Ironically enough, there is bound to be some distortion introduced by the fact that many House candidates are incumbents! The extent to which this is true will vary somewhat among states and between parties, to be sure, but the problem is nonetheless a real one.[42] Some correction is afforded by choosing the worst performance out of five elections, for even though most incumbents will probably *win* in trying times, their *share* of the vote is probably diminished, and it is the percentage received rather than the won-loss record that is the relevant datum.

The second problem with the measure is that of turnout. More people vote in presidential than in off-year elections; furthermore, the types of people voting differ between election years. These differences are likely to introduce distortion into any kind of measure of "party vote" based on actual votes. Yet the problem is less serious in fact than its potential suggests. For a study by Harvey Kabaker designed to test the extent to which turnout does actually influence the partisan division of the vote *in congressional elections* concludes that the effect is really quite minimal.[43] Hence there is reason to have some confidence that this indicator of "base party vote" is in fact measuring what our conceptual apparatus says it should.

Choosing a variable for incumbency is a simple operation. If a candidate is the current office-holder, and has achieved the position by virtue of an *election*, he is considered the incumbent. An election, rather than an appointment, is required because a principal source of strength for an incumbent, as argued above, is the political skill, organization, personal following, and recognition that results from having waged a successful campaign. In addition, appointees are sometimes selected to fill a vacancy precisely because they pose no real political threat to the governor appointing them; yet the governor can use the opportunity to reward favors bestowed. Evidence of the role of these factors is the lower re-election rate experienced by appointees as compared with those who have been elected, even in a special election. Once ascertained that the candidate meets the criterion set forth here, the variable I is set equal to 1; if the candidate is not the incumbent, $I=0$.

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[40] At the end points of the election series, the following modifications prevailed: the "base party vote" for 1948 was selected from the period 1946–1952; that for 1968 from 1964–1970; and that for 1970 from 1966–1970.

[41] The concept is developed and explicated by Philip E. Converse in "The Concept of a Normal Vote," in *Elections and the Political Order,* Campbell et al., pp. 9–39.

[42] An extreme case is that of Nevada, where the "base party vote" for the Democrats exhibited an exceptionally strong and consistent increase during most of the postwar period. Whether this is due to a "true" growth of Democratic loyalists or to the fact that there is only one Representative who, having been the incumbent since 1956, has been able to steadily tighten his hold on the electorate over the last decade and a half, is a moot question. The potential for distortion in such a situation is clear.

[43] Harvey M. Kabaker, "Estimating the Normal Vote in Congressional Elections," *Midwest Journal of Political Science,* 13 (February, 1969), 58–83.

The "national tides" variable must be chosen to reflect the way a given state has responded to the general trend in the nation. Therefore, the "tide" for a particular party in a particular year in a given state is measured by the percentage of the votes received by that party's House candidates in the present election year minus the percentage received the immediately preceding election year.

The idiosyncratic factors are not measured directly. They are estimated with the constant term (the intercept) of the equation which results from directly estimating the rest of the parameters of the model through regression.

In using these variables to estimate the parameters of the model through multiple regression, we separate by party and by year.[44] That is, there are separate estimates of the relative importance of the phenomena contained in the theory for each party and for each election year from 1948 to 1970—twenty-four sets of estimates in all (two parties times twelve election years).

An overall model for each Senate "class" was also estimated, separating by party as before. In other words, all the Democratic party election data for each year from 1948 to 1952 were collected into one set in order to get one (slightly more stable, there being more observations) estimate, and the same was done for the Republicans. The procedure was repeated for each subsequent six-year sequence. The obvious rationale here is that in each six-year period all Senate seats will have been contested once and only once in a general election. Each period is designated as follows: Group 1, 1948–1952; Group 2, 1954–1958; Group 3, 1960–1964; and Group 4, 1966–1970. Let us now turn to the analysis of these estimates.

Results and Analysis: Grouped Data. The main focus in this analysis is on (1) the relationship between party and incumbency in Senate elections both within and between the two major political parties, and (2) changes in those relationships during the postwar period. The primary purpose of the "tides" variable is to remove certain confounding elements from the relationship between the two phenomena of central interest. Thus, although we will report its values and discuss its interpretation in the hypothetical data example below, it should be considered, for the most part, a "control variable," whose function is to control for otherwise distorting effects.

The results of the multiple regression on the grouped data are reported in Table 7. Both the actual parameter estimates (including the constant term) and the standardized regression coefficients, or beta-weights, are presented. I will employ the former to illustrate the use of these results as actual prediction equations. However, because the variables have somewhat different distributions between parties in a given election year (or Group), and even more varied distributions over time, the similarities and contrasts between the parties and over time will be analyzed in terms of the standardized coefficients.

Let us first describe the use of these equations in a hypothetical prediction situation. Recall that the model is

$$[(9)\text{ repeated}]\quad S=a(P)+b(I)+c(T)+Id,$$

where a, b, and c have now been estimated from data by multivariate statistical techniques. When the estimates for Group 3 (1960–64) are used, for example, the model for the Democrats reads

$$\hat{S} = 0.54\,(P) + 0.09\,(I) + 0.50\,(T) + 0.23, \tag{10}$$

and for the Republicans it reads

$$\hat{S} = 0.40\,(P) + 0.10\,(I) + 0.58\,(T) + 0.26, \tag{11}$$

where S, P, T, and Id (the constant term) are expressed in proportions, not percentages. If in 1964 the "base party vote" for the Democrats in a given state was 0.485 (i.e., 48.5 per cent) and the Democratic "national tide" in that state that year was +0.05, we would "predict" that a Democratic senatorial candidate who was the incumbent ($I = 1$) would receive

$$0.607 = 0.54(0.485) + 0.09(1) + 0.50(+0.05) + 0.23, \tag{12}$$

or 60.7 per cent of the vote. Note that being an incumbent was "worth" nine per cent.

If in the same state that year the Republican "base party vote" was 0.475 (suggesting that the two parties were highly competitive), the

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[44] Uncontested Senate elections were omitted from the analysis. Cases where the Representatives from a state were unopposed eliminated that state from the analysis for that year (i.e., the "tides" variable was considered to have a missing value for that year, which required deletion of the entire state). Such an occurrence was quite infrequent. All statistical computations were carried out on Washington University's IBM 360/50 via the *Statistical Package for the Social Sciences* (SPSS) routines.

Table 7. Multiple Regression Estimates of the Effects of Party, Incumbency, "National Tides," and Idiosyncratic Factors on Senate Elections, Grouped Data: 1948–1970

	Democrats							
Years	Parameters				Beta-Weights			R^2(%)
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948–52	.72	.044	.62	.16	.71	.19	.36	70.2
(1)[a]	(.07)[b]	(.02)	(.11)					
1954–58	.66	.07	.36	.19	.61	.36	.22	67.2
(2)	(.09)	(.02)	(.12)					
1960–64	.54	.09	.50	.23	.39	.45	.31	57.6
(3)	(.11)	(.02)	(.13)					
1966–70	.10	.12	.29	.40	.08	.58	.19	50.2
(4)	(.12)	(.02)	(.14)					

	Republicans							
Years	Parameters				Beta-Weights			R^2(%)
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948–52	.71	.01	.46	.17	.72	.07	.28	62.7
(1)	(.08)	(.02)	(.12)					
1954–58	.58	.06	.17	.19	.53	.31	.10	49.4
(2)	(.10)	(.02)	(.14)					
1960–64	.40	.10	.58	.26	.34	.51	.36	61.6
(3)	(.09)	(.02)	(.12)					
1966–70	.15	.11	.40	.39	.12	.50	.27	43.3
(4)	(.11)	(.02)	(.13)					

[a] Data are grouped into "classes" of three consecutive election years. Number in parentheses denotes the Group or "class."

[b] Standard error of the parameter.

senatorial candidate was not, of course, the incumbent ($I = 0$), and the Republican "national tide" was equal in size but opposite in sign to the Democratic (i.e., -0.05). the model "predicts" for that candidate

$$0.421 = 0.40(0.475) + 0.10(0) + 0.58(-0.05) + 0.26, \tag{13}$$

or 42.1 per cent. Note that if the Republican were the incumbent instead of the Democrat, we would predict victory for the GOP candidate. Also, observe that party and incumbency, when both are present (i.e., $I=1$), are more substantial sources of support than are the idiosyncratic factors represented by the constant term. Finally, notice that if neither candidate was the incumbent, the Democrat would still win, but with slightly more than a majority of the total vote.

One of the reasons the Group 3 results were selected to exemplify the use of the models as prediction equations—and to illustrate thereby the substantive interpretation of the theory underlying the model—is that the parameters are all very stable. By this is meant that the coefficients are all more than twice the size of their standard errors. In terms of "statistical significance," this means that we can reject in each case the null hypothesis (i.e., that there is no linear relationship between the independent variable and the proportion of votes received by the senatorial candidates) with less than a five per cent chance of error. But a "statistical significance" interpretation is not strictly applicable here for it is a population that is involved rather than a sample, namely, the universe of all Senate elections from 1948 to 1970. Nevertheless, the relationship between the size of the coefficients and their standard errors does give some indication both of the basic stability of the estimates and of the extent to which there is in fact a non-zero relationship between each independent variable and the dependent variable. Consequently, the results will be discussed in this light.

In only two instances is a parameter patently indistinguishable from zero, that is, in only two instances is the standard error *larger* than the coefficient itself. These occur with the Group 4 Democratic estimate of the effect of party, and the Group 1 Republican estimate of the effect of incumbency. As shall be seen, neither case causes any real problem of substantive interpretation, since the actual estimate of these effects should be nearly zero anyway.

Two other estimates, both in Republican "Classes," are somewhat unstable, viz., the Group 4 party coefficient and the Group 2 "tides" coefficient. These are only "somewhat unstable" because although the coefficient is not twice the size of its standard error, it is still the larger of the two. Fortunately, the party coefficient again causes no real problem substantively, as will become evident below. Nevertheless, the small size and relative instability of the Republican "tide" is curious given what we know about 1958. Yet since its function is that of a control variable, we need not concern ourselves here with its behavior.

Turning to the first of the two substantive questions at hand, examination of the beta-weights reveals no particularly outstanding incongruity *between parties* in the relative importance attaching to the various factors in each Group. The only relatively consistent difference between the party models is the greater amount of variance explained (R^2) for the Democrats than for the Republicans.[45]

Far more interesting, though, is the response provided by the models to the second question of central concern: What has been the direction and magnitude of the *change* in the relative importance of party and incumbency, for both political parties, in Senate elections of the postwar period? The answer, shown schematicaally in Figures 4 and 5, is as dramatic as it is unequivocal: The importance of party has undergone a sharp, secular decline, while the importance of incumbency has experienced an almost commensurate increase. There are only two notable differences between the parties. For one, the magnitude of the predominance of incumbency over party for the Republican "Class" of 1960–1964 is almost three times that for the Democrats, although the direction of the difference is the same in both cases. Second, while the *decline* of party is unquestionably monotonic and nearly linear for both parties, the *increase* in the importance of incumbency continues unabated over the whole period only for the Democrats; the increase for the Republicans, more rapid during the first dozen years, levels off for the last three elections.

Inspection of the "constant" term and the R^2 column of Table 7 shows them to be consistent with the theoretical and substantive implications of these patterns. As party has decreased in influence, and incumbency has taken up part, but not all, of the "slack," the role of idiosyncratic factors in Senate elections has steadily grown. Examination of the R^2 column of the same Table bears this out since, for the Democrats especially but for the Republicans as well, the ability to predict by relying on the two principal systematic factors (plus "national tides") has been appreciably reduced. The difference for both parties between the amount of variance explained by the models in the early part of the postwar period (1948–1952) and the amount explained in the last part (1966–1970) is approximately 20 per. cent. The variance explained is still quite high, to be sure, but the drop is nonetheless considerable.

Analysis of Results: Ungrouped Data. The same general conclusions can be drawn from an analysis of the ungrouped, election year-by-election year data. The parameter and beta-weight estimates for each party are recorded in Table 8, with the values of the standardized regression coefficients for party and incumbency depicted in Figures 6 and 7. Once again there is a marked overall decline in the relative importance of party, and a concomitant increase in the relative importance of incumbency. Setting 1956 aside as a "deviant case" for the Republicans would accentuate the steadiness of these trends, while doing the same with 1958 for the Democrats would have a similar effect. In addition, the constant term and R^2 values follow the expected pattern. The influence of idiosyncratic factors has increased by an average of roughly one per cent each biennial election, and the amount of variance explained has

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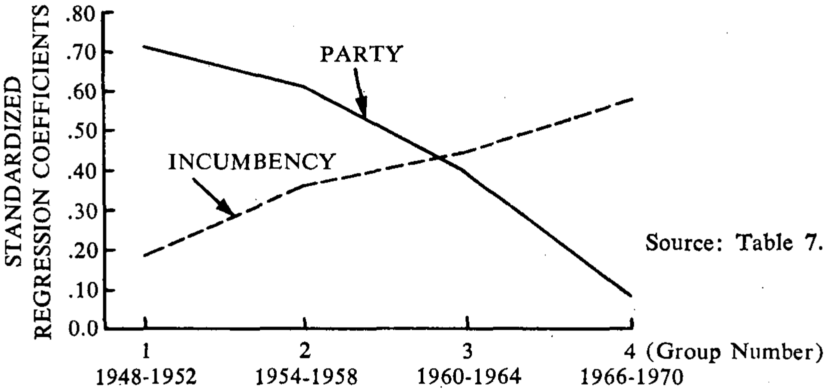
[45] Yet this should not lead us to conclude that the parties are in any important sense "the same," for the similarity may very well be due mainly to the nature of the variables employed. The Republican S-values are almost always complements of the Democratic S-values. Although the P-values for the two parties are certainly not linear transformations of each other, there is nonetheless enough of an inverse relationship between them (and between the incumbency variables for the two parties) to make the similarity in behavior of the separate party equations as much of a technical expectation as of a substantive finding. I am grateful to an anonymous referee for this observation.

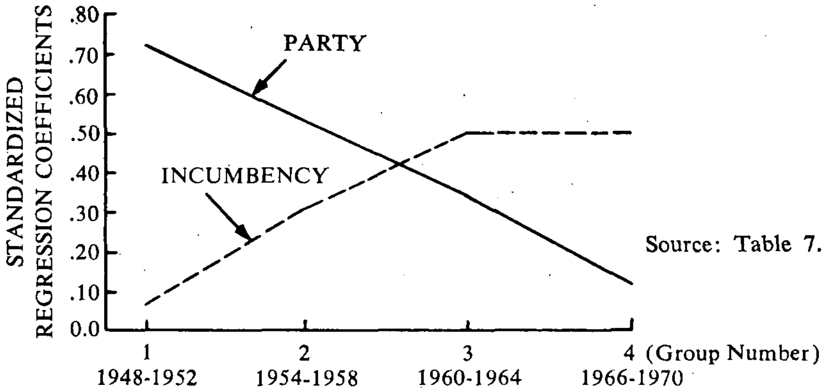


Source: Table 7.

Figure 4. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections, from a Model Employing "National Tides" as a Control Variable: Data Grouped into "Classes" of Three Consecutive Election Years Each: Standardized Regression Coefficients: Democrats, 1948–1970

fallen by a total of almost 40 per cent during the quarter-century covered by the analysis, a drop twice the size encountered in the grouped data.

Still, it must be admitted that the ungrouped data reveal somewhat more complex phenomena at work than do the grouped data. Either set of data permits us to assert the clear predominance of party in Senate elections during the period 1948–1952; the ungrouped data extend that conclusion to 1954 for the Republicans, to 1956 for the Democrats. Thereafter, however, the interpretations based on the grouped data require at least three important qualifications: (1) While the overall decline in the importance of party did continue over the entire period for both parties, the Republican decline from the late 'fifties onward certainly lacks the urgency suggested by the grouped data results (cf. Figure 5). (2) For both parties, incumbency has *not* shown a strong *consistent* increase in importance from the late 'fifties onward. This finding was already apparent for the Republicans from an analysis of the grouped data, but constitutes a modification of the conclusion of the earlier analysis regarding the Democrats. Although both sets of results attach greater contemporary importance to incumbency than to party for Democratic senatorial candidates, the ungrouped data suggest that this is due less to the continued overall increase in the incumbency trend reported in Figure 5 and more to the continued overall decline in the party trend. (3) The general pattern of the late 'fifties and the subsequent decade is one of fluctuation in relative importance from election to election, rather than the persistent dominance of either incumbency or party. In fact, in many elections it is impossible to determine whether one factor really exerted more influ-



Source: Table 7.

Figure 5. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections, from a Model Employing "National Tides" as a Control Variable: Data Grouped into "Classes" of Three Consecutive Election Years Each: Standardized Regression Coefficients: Republicans, 1948–1970

Table 8. Multiple Regression Estimates of the Effects of Party, Incumbency, "National Tides," and Idiosyncratic Factors on Senate Elections: 1948–1970

Republicans

Year	Parameters				Beta-Weights			R^2(%)
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948	.68 (.10)[a]	.01 (.03)	.83 (.36)	.20	.88	.06	.29	71.8
1950	.78 (.13)	.03 (.029)	.28 (.31)	.28	.75	.13	.10	73.2
1952	.92 (.15)	.01 (.02)	.28 (.26)	.07	.80	.07	.12	69.1
1954	.64 (.09)	.05 (.02)	−.39 (.24)	.17	.72	.21	.16	88.0
1956	.20 (.22)	.08 (.03)	.30 (.24)	.37	.17	.49	.20	44.0
1958	.57 (.15)	.07 (.03)	−.09 (.31)	.14	.54	.34	−.04	51.9
1960	.39 (.14)	.10 (.02)	.25 (.23)	.28	.40	.57	.14	66.4
1962	.42 (.12)	.10 (.03)	.29 (.20)	.26	.46	.51	.16	65.8
1964	.47 (.15)	.11 (.03)	1.12 (.29)	.25	.43	.44	.52	55.0
1966	.32 (.17)	.13 (.03)	−.03 (.26)	.33	.30	.59	−.02	51.5
1968	.47 (.14)	.08 (.038)	.72 (.32)	.26	.46	.28	.30	55.1
1970	.34 (.15)	.12 (.05)	−.08 (.35)	.25	.35	.38	−.03	31.3

Democrats

Year	Parameters				Beta-Weights			R^2(%)
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948	.63 (.08)	.06 (.03)	1.04 (.28)	.18	.84	.20	.37	79.4
1950	.69 (.10)	.04 (.03)	.40 (.25)	.16	.75	.17	.16	78.4
1952	1.06 (.13)	−.01 (.03)	.19 (.24)	.01	.88	−.04	.09	80.6
1954	.56 (.10)	.06 (.02)	−.58 (.23)	.27	.62	.23	−.25	88.9
1956	.57 (.08)	.06 (.02)	.51 (.17)	.23	.72	.28	.27	83.8
1958	.37 (.17)	.15 (.03)	.29 (.23)	.32	.28	.65	.13	72.8
1960	.77 (.24)	.08 (.036)	.02 (.28)	.12	.52	.36	.01	62.6
1962	.80 (.13)	.07 (.02)	−.12 (.19)	.11	.66	.34	−.07	69.0
1964	.41 (.17)	.10 (.03)	.59 (.25)	.29	.35	.51	.33	52.1
1966	.21 (.16)	.14 (.03)	.04 (.21)	.34	.19	.68	.03	56.3
1968	.36 (.21)	.08 (.037)	.54 (.44)	.31	.28	.36	.20	40.9
1970	.39 (.19)	.12 (.04)	.02 (.34)	.29	.31	.46	.01	34.5

[a] Number in parentheses is the standard error of the coefficient.

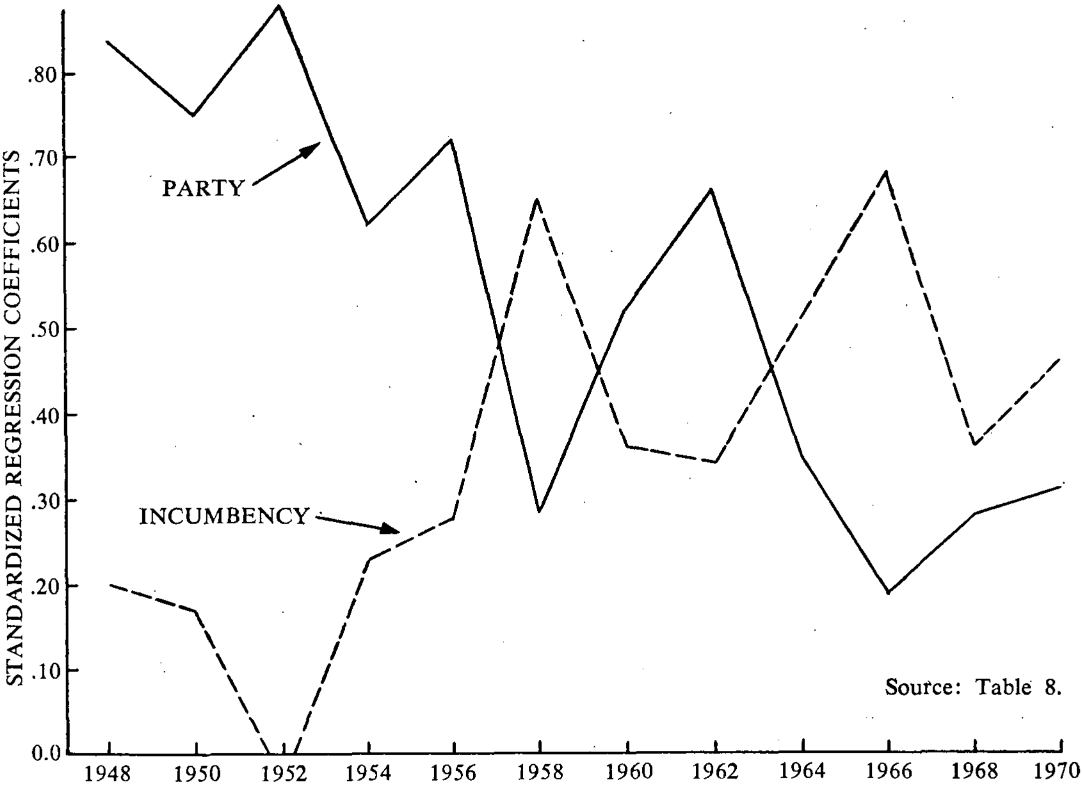


Figure 6. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections in Each Election Year from 1948 through 1970: Standardized Regression Coefficients from a Model Employing "National Tides" as a Control Variable: Democrats

ence than the other because the difference between them is so small. This difficulty is attenuated in 1962 and 1966 for the Democrats, and in 1966 for the GOP, but remains for the other years.

The stability of the estimates based on the ungrouped data is not as adversely affected by the reduction in the number of observations as might have been expected. The exception is the "tides" variable, for which the standard error is larger than the coefficient 8 out of 24 times. But, as noted before, this need not concern us, given the function the variable serves.

Such great instability occurs only three times for the incumbency variable, and only once for party. The instability of the incumbency variable causes no theoretical problems since it happens early in the period; even the very stable estimates of the influence of incumbency during those election years are very small. The standard error, however, is vexingly large for the party coefficient in 1956, near the middle of the election series. But even here the substantive interpretation is a plausible one. We know that the outpouring of support for Eisenhower was far from a strict "party vote"; that is, many non-Republicans voted for the President's reelection. Although Eisenhower's "coattails" were not considered particularly long that year, the size of the "tides" component in our model (fourth-largest in the entire Republican series) indicates that they did in fact have some effect on the Senate elections. Many of those who voted for Eisenhower voted for the Republican candidate for the Senate as well. Nevertheless, the principal influence on the electoral support given Republican senatorial candidates that year was the congeries of idiosyncratic elements discussed previously. They probably were able to come into play, of course, partly because of the "loosening" effect of the strong "presidential tide." The only other times the standard error of the party coefficient approaches the size of the coefficient itself are

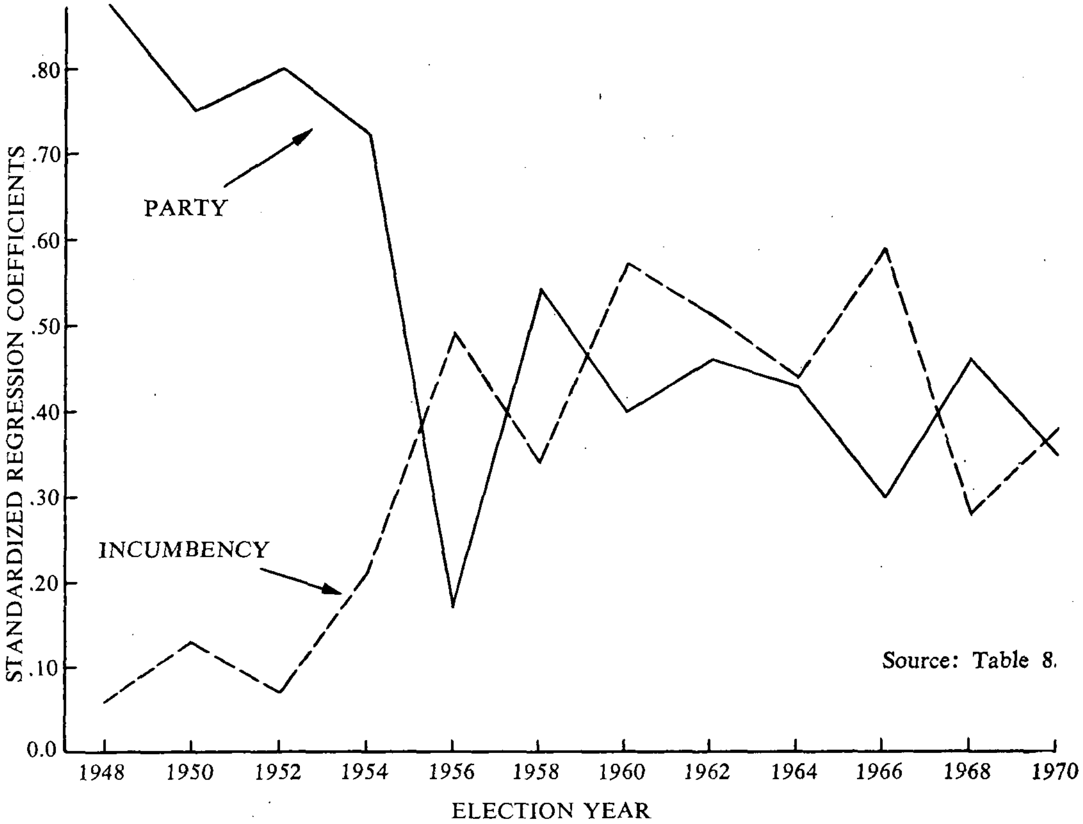


Figure 7. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections in Each Election Year from 1948 through 1970: Standardized Regression Coefficients from a Model Employing "National Tides" as a Control Variable: Republicans

three instances in the late 1960s, again posing no real problem of interpretation, given the overall pattern.

Summary and Implications. The major finding is that the relative importance of party and incumbency in Senate elections has changed dramatically over the last quarter-century. Party has undergone an overall decline in influence, while incumbency has experienced a roughly proportionate increase. At the same time, the importance of idiosyncratic factors has grown. These patterns are virtually identical for both major political parties. Naturally, these trends have been accompanied by a substantial decrease in the (statistical) explanatory ability of the theory and models employed in the analysis.

These findings fit well with previous research documenting the present malaise in the capacity of "party" to structure both mass electoral behavior and congressional voting behavior.[46] Clearly a crucial linkage between electors and elected has been weakened. This study indicates, however, that incumbency now serves, at least in Senate contests, as an important alternate voting cue to party, thereby assuming some of the linkage function formerly performed by party. But the increase in the structuring role played by incumbency has not been sufficient to offset the substantial decline in party's ability to play that role. The consequence, of course, is a more tenuous, less regularized overall relationship between the voters and candidates for the U.S. Senate. This situation of relative disarray is reflected in the present analysis in the increased importance of idiosyncratic factors and in the decreased explanatory power of the models employed.

The implications of these findings for more general theories of American politics are widereaching and profound. For example, they address quite directly the ongoing debate over the existence of recurring periods of "critical realignment" in American electoral history.[47] Some scholars have argued that the United States is presently in the midst of such a realignment sequence. While the results of the present study do not necessarily contradict such an interpretation, they do disagree with certain areas of the argument. One such area involves the timing of the decline in the relative power of party to influence electoral behavior. Some studies date a precipitate drop in the power of party as recently as the mid-1960s, citing, among other things, the sharp increase at that time in the proportion of those who identify themselves as "independents."[48] While such evidence is highly suggestive, the present data strongly suggest that, at least in Senate elections, the decline has been far more steady and long-term. Indeed, the effect of party has been muted during the entire postwar period. Thus, at the very least, this finding would seem to demand a fresh look at the subpresidential manifestations of such realignment phenomena.

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[46] See, e.g., Gerald M. Pomper, Badi G. Foster, Charles E. Jacob, Wilson C. McWilliams, Alan Rosenthal, and Jay A. Sigler, *The Performance of American Government* (New York: The Free Press, 1972), pp. 81–86.

These findings have implications as well for a number of fundamental problems of democratic theory. For example, the switch in the relative strengths of party and incumbency in influencing Senate elections bears on basic questions of the representativeness and responsiveness of American democratic institutions. For if "party" is no longer the controlling force it once was in determining electoral outcomes, and if representatives are more beholden to incumbency for their success at re-election, then new definitions of the relationship between voters and elected officials may be required. The nature and quality of the electoral and policy linkage provided by each of these two phenomena, party and incumbency, obviously demand careful, sustained attention.

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[47] The most notable exponent of such an interpretation is Walter Dean Burnham. See his "The Changing Shape of the American Political Universe," *American Political Science Review,* 59 (March, 1965), 7–28, and his more recent and comprehensive *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970). The concept of a "critical election" was first given academic sanction in V. O. Key, Jr., "A Theory of Critical Elections," *Journal of Politics,* 17 (February, 1955), 3–18. Also pertinent are E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart, and Winston, 1960); Gerald M. Pomper, *Elections in America* (New York: Dodd, Mead and Company, 1968); *The American Party Systems,* ed. William Nisbet Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967); among others.

[48] See, e.g., Burnham, *Critical Elections.* Erikson also sees the mid-1960s as significant. See his "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," at pp. 1240 ff.

The Paradox of Vote Trading*

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The conventional judgment on vote trading in legislatures is one of severe disapproval. In the United States, at least, the usual idiom is "logrolling" and this word has always had pejorative connotations.[1] The commonsense of the language has been reinforced by the judgment of scholars. Schattschneider, whose study of the writing of the Smoot-Hawley Tariff of 1930 is the most detailed examination of a single event of vote trading, concluded his book with the remark: "To manage pressures [i.e., for vote trading] is to govern; to let pressures run wild is to abdicate."[2] Furthermore, it seems likely that the widespread scholarly support for concentrated national leadership—as embodied, for example, in such writings as the report of the Committee on Political Parties of the American Political Science Association, a report to which Schattschneider contributed substantially—was engendered in part at least by a desire to minimize vote trading.[3]

Despite the long-standing unanimity of scholarly and popular judgment, however, it has recently been argued by a number of scholars that vote trading is socially desirable because it allows the expression of degrees of intensity of preference. Buchanan and Tullock argue that the voter or legislator can improve his welfare "if he accepts a decision contrary to his desire in an area where his preferences are weak in exchange for a decision in his favor in an area where his feelings are stronger." Hence they say: "Bargains among voters can, therefore, be mutually beneficial."[4] The authors recognize that, when bargains are concluded "in which the single voter does not participate, . . . he will have to bear part of the costs of action taken."[5] But they nevertheless conclude that these external costs are on the average less than the benefits obtained so that for the society as a whole vote trading or logrolling is a desirable kind of event.

Following this argument there has developed a small literature on the subject that interprets the market for votes as if it were a market for goods. Tullock has elaborated his earlier position.[6] Coleman has argued that logrolling is a device both to avoid Arrow paradoxes and to arrive at optimal allocations.[7] A number of writers have pointed out, however, that vote trading does not eliminate these paradoxes.[8] But these same writers and others have taken pains to argue that it does improve allocations in the direction of Pareto optimality.[9]

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* An earlier version of this paper by the senior author was originally presented at the Annual Meeting of the American Political Science Association, Washington, D.C., September 5–9, 1972.

[1] *A Dictionary of Americanisms* (1951), which is probably the most authoritative collection, cites many political examples from 1812 (Ninian Edwards) to 1949 (*Time* magazine), in every one of which the word is clearly used in a pejorative sense. In the quotation from Edwards, logrolling is specifically identified with intrigue; and the climate of intrigue is conveyed by almost all the later quotations. The Oxford Dictionary (i.e., *A New English Dictionary on Historical Principles*) cites political usage from both sides of the Atlantic from 1823 (*Niles Weekly*) on to the end of the century, and its quotations also are uniformly pejorative. Bartlett's *Dictionary of Americanisms* (1859) contains a lengthy and righteously indignant explication of the political sense of "logrolling." None of these sources indicate any pejorative connotation at all for the word when it refers to clearing land or building houses. It must have acquired the sense of intrigue, therefore, only when it was transferred from the bartering of labor to the bartering of votes. And this demonstrates that the bartering of votes has regularly been socially disapproved.

[2] E. E. Schattschneider, *Politics, Pressures and the Tariff* (New Jersey: Prentice-Hall, 1935), p. 293. Schattschneider interpreted the process of tariff writing as an "attempt to set up a beneficently discriminatory set of privileges"—beneficient in the sense that a few particular industries deemed worthy would benefit. This attempt failed, he argued, because the legislation as actually written protected so "indiscriminately . . . as to destroy the logic and sense of the policy" (p. 283). One of the purposes of this essay is to explain the conditions for such failure.

[3] Committee on Political Parties of the American Political Science Association, *Toward a More Responsible Two-Party System* (New York: Rinehart, 1950).

[4] James Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962), p. 145.

[5] *Ibid.*

[6] Gordon Tullock, "A Simple Algebraic Logrolling Model," *American Economic Review*, vol. 60 (June, 1970), pp. 419–26.

[7] James Coleman, "The Possibility of a Social Welfare Function," *American Economic Review*, vol. 56 (December, 1966), 1105–22.

[8] Dennis C. Mueller, "The Possibility of a Social Welfare Function: Comment" *American Economic Review*, vol. 57 (December, 1957), 1304–11; and, in an especially lucid and compelling argument, Robert Wilson, "An Axiomatic Model of Logrolling," *American Economic Review*, vol. 59 (June, 1969), 331–41, shows that, except in one unlikely circumstance, logroll-

We have thus an intellectual confrontation between an older (and popular) tradition and new developments in social science. In this essay, we intend to reconcile the conflict. We will show (1) that logrolling itself (as if in a legislative market for votes) does in fact improve the position of the traders; but (2) we will show also that this very improvement generates external costs for the non-traders—costs that may be—and probably sometimes are—of such magnitude that they generate a paradox of vote trading. This paradox has the property that, while each trade is individually advantageous to the traders, the sum of the trades is disadvantageous to everybody, including the traders themselves. Furthermore, since trading is individually rational, this paradox is inescapable, given an appropriate distribution of taste. Even if the original trades are not considered binding so that traders can try to extricate themselves from the paradox, they may make new trades which result in an indefinite cycle of return to the original position, return to the paradox, etc. Nor can this lack of a stable equilibrium be compensated for by the formation of coalitions. So truly the paradox, once generated, is inescapable. In general, therefore, the market for legislative votes is quite different from the market for private goods and it is not wise to draw analogies from one market to the other.

A Model of Vote Trading

In order to analyze some of the essential features of vote trading, it is useful to establish an abstract legislature consisting of the set N of members, $N=\{1, 2, \cdots, n\}$. This legislature deals with motions, $x, y, \cdots$, which are proposals to impose commands. The set of motions, A, is indefinitely large, $A=\{x, y, \cdots\}$. If a motion, x, is adopted, then an associated outcome, X, exists, and X is the state of affairs in which the command in x is imposed. If, however, a motion, x, is rejected, then the associated outcome $\tilde{X}$ exists, and $\tilde{X}$ is similar to the status quo ante except that it contains the memory of the decision not to impose the command in x. Hence, associated with the set of motions, A, is a set of possible outcomes, 0, which is twice the size of A and is also indefinitely large, $0=\{X, \tilde{X}, Y, \tilde{Y}, \cdots\}$.

In this abstract legislature, motions are made and voted upon serially, so that future motions are not necessarily anticipated when current motions are considered. Note that this assumption about the system is markedly different from the assumption made in the essays cited in notes 8 and 9, where it is typically laid down that all motions are simultaneously before the legislature. In such a system support can be traded back and forth in all possible ways, but in the present system support can be traded only with respect to the subset of A currently before the legislature or anticipated to come before it. The rationale for this more restrictive assumption is simply that it accords with a crucial feature of the real world. In the third section, however, we remove this restriction only to find that it makes no theoretical difference, although it may make a practical difference.

The legislators produce outcomes from motions by voting, where votes are defined thus:

$v_i(x)=1$ is a vote cast yea by member i on motion x
$v_i(x)=-1$ is a vote cast nay
$v_i(x)=0$ is a vote not cast (i.e., an abstention)

The rule connecting motions to outcomes is the usual one of simple majorities:

$$\text{if } \sum_{i=1}^{n} v_i(x) > 0, \qquad \text{then } X \text{ exists}$$

$$\text{if } \sum_{i=1}^{n} v_i(x) \leq 0, \qquad \text{then } \tilde{X} \text{ exists.}$$

The members of N order the elements of the set 0 by a relation, R, which is connected, transitive, and reflexive.[10] The relation, R, may be thought of as a combination of the relation of preference, P, and of indifference, I. The member, i, who holds the relation is, if necessary, identified by a subscript, R_i. The entire set 0 may be ordered by R and from that ordering cardinal utility numbers, $u_i(X)$, can be derived. Finally, it is assumed that the people in N are rational in the sense that they act to maximize utility or, if appropriate, expected utility.

It is important to note that utility is defined over outcomes, which are the goals themselves, not over motions, which are merely the means to the goals. While it is probably the case that most people care about the alternative goals rather than the means, the use of goals in the definition is not

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ing necessarily involves a violation of Arrow's condition of independence from irrelevant alternatives.

[9] See Wilson, *op. cit.;* Edwin T. Haefle, "Coalitions, Minority Representation, and Vote-Trading Probabilities," *Public Choice,* vol. 8 (Spring, 1970), 75–90; and Dennis C. Mueller, Geoffrey C. Philpotts, and Jaroslav Vanek, "The Social Gains from Exchanging Votes: A Simulation Approach," *Public Choice,* vol. 13 (Fall, 1972), 55–79.

[10] To say R is connected means that either XRY or YRX is true; to say R is transitive means it is true that if XRY and YRZ, then XRZ; to say R is reflexive means that it is true that XRX.

the consequence of a passion for realism. Instead utility is defined over goals in order to allow for voting against motions that lead to the achievement of one's immediate interest—which is the essence of logrolling. Suppose utility were in fact defined over motions, i.e., $u_i(x)$, rather than $u_i(X)$. Then if $u_i(x)>u_i(\tilde{x})$, it would necessarily and invariably be that, among rational actors, $v_i(x)=1$. That is, if the goal itself were the motion, then one would always have to vote in accordance with one's true tastes. But if the motions are merely means, then it is conceivable that, even though $u_i(X)>u_i(\tilde{X})$, still $v_i(x)=-1$ because member i is achieving some other goal by devious means. In short, defining utility over outcomes rather than motions allows for the possibility of devious behavior such as is often involved in vote trading.

Without, we hope, a loss of generality, it is from now on assumed that all members of N are concerned about motions and that, as between X and $\tilde{X}$, either $XP_i\tilde{X}$ or XP_iX. There is not much loss of realism in this assumption if we think of real legislatures as varying in size from decision to decision according to the number of legislators who take part in them.

There is a set $S(x)$ and its complement $-S(x)$ which are pairwise disjoint subsets of N so that $S(x)\cup -S(x)=N$ and $S(x)\cap -S(x)=\varnothing$; where $\varnothing$ is the empty set. $S(x)$ has s members and $-S(x)$ has $(n-s)$ members; and $n<2s$ so that $S(x)$ is always larger than $-S(x)$. The defining property of $S(x)$ is that all the members of S have the same preference between X and $\tilde{X}$ and all the members of $-S(x)$ have the opposite preference. That is, if i, $i\in S(x)$, has $XP_i\tilde{X}$, then j, $j\neq i$ and $j\in S(x)$, also has $XP_j\tilde{X}$ and k, $k\in -S(x)$, has $\tilde{X}P_kX$; while, if i has $\tilde{X}PX$, then j has $\tilde{X}PX$ and k has $XP\tilde{X}$. Thus $S(x)$ and $-S(x)$ may be thought of as the majority and minority that would exist on motion x if everybody who preferred X to $\tilde{X}$ voted yea on x and everybody who preferred $\tilde{X}$ to X voted nay on x.

There is in addition a set $T(x)$ and its complement $-T(x)$ which are also pairwise disjoint subsets of N so that $T(x)\cup -T(x)=N$ and $T(x)\cap -T(x)=\varnothing$. $T(x)$ has t members and $-T(x)$ has $n-t$ members; and, again, $n<2t$ so that $T(x)$ is always larger than $-T(x)$. The defining property of $T(x)$ is that all members of T vote the same way on x and all members of $-T$ vote the opposite way. That is, if $v_i(x)=1$, then for all j, $j\neq i$, $j\in T(x)$, $v_j(x)=1$ and for all k, $k\in -T(x)$, $v_k(x)=-1$, while if $v_i(x)=-1$, then $v_j(x)=-1$ and $v_k(x)=1$. Thus, $T(x)$ and $-T(x)$ are the actual majority and minority on a vote on motion x.

If a voter i always voted for x when $XP_i\tilde{X}$ and against x when $\tilde{X}P_iX$, then $S(x)$ and $T(x)$ would be identical. This is what is known as "voting in accord with one's true tastes" or "sincere" voting. It might just as reasonably be called naive.[11] Regardless of name, sincere or naive voting is to be contrasted with sophisticated voting, which is defined as voting against one's immediate interest when there is an incentive to do so. That is, if $u_i(Y)>u_i(X)$ and $u_i(X)>u_i(\tilde{X})$ and if i can achieve Y by voting against x, then $v_i(x)=-1$ is sophisticated voting. There are several reasons why one might choose to do so (e.g., to exploit the fact of a voting paradox), some of which are analyzed by Black and Farquharson. Surely the main reason for sophisticated voting is logrolling and this is what will be analyzed here.

To vote sophisticatedly is to switch sides between $S(x)$ and $T(x)$. If one is in $S(x)$ and orders $XP\tilde{X}$, to switch is to resign from the set $S(x)$ and join $-S(x)$ where one acts as if one orders $\tilde{X}PX$. Hence for some i_0, who is a particular member of $S(x)$, switching is:

$$S(x) - \{i_0\}$$
$$-S(x) + \{i_0\}.$$

Of course, i_0 may be any number of persons, but for convenience the symbol is treated as if it represented a single person.

Under what conditions might switching be expected to occur? Surely one necessary condition is that the switcher belong to $S(x)$. The motive for switching is to bring about a result different from that which would occur if voting were sincere. Assume that some i_0 switches from $-S(x)$ to $S(x)$, then the result is unchanged. The preferred outcome of the members of $S(x)$ was the winner before the switch and remains so after it. If, however, i_0 (hereafter identified simply as "i" without the particularizing subscript) switches from $S(x)$ to $-S(x)$, it may be that the preferred outcome of the members of $S(x)$ loses. Hence the only chance that a member has to change the outcome is when he belongs to the initial majority or to the majority under sincere voting. The first condition for switching is, for the switcher, i, that:

$$i\in S(x). \tag{1}$$

Elaborating the same notion, the switcher actually changes the outcome only if he is a pivot, only if, that is, his switching renders the initial winner a loser and the initial loser a winner. This condition can be expressed in a stronger way: In order to justify switching, the switcher must be compensated by a specifiable amount. (If he switches without any compensation for his act of giving up his position on the side he himself favors, his behavior must be regarded as truly irrational and self-destructive.) Since, for the switcher, $u_i(X) > u_i(\tilde{X})$ while he votes $v_i(x) = -1$ or $u_i(\tilde{X}) > u_i(X)$ while he votes $v_i(x) = 1$, his vote costs him the absolute value of $|u_i(X) - u_i(\tilde{X})|$. To induce him to bear that cost, there must be available in the system an amount b that is larger than the cost:

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[11] Duncan Black, *The Theory of Committees and Elections* (Cambridge, Cambridge University Press, 1958), p. 44, uses the former phrase and Robin Farquharson, *Theory of Voting* (New Haven, Yale, 1970), pp. 17–18, uses the latter. Voting in accord with one's true tastes (defined over motions) seems "sincere." But if utility is defined over outcomes, it seems merely naive to confuse means and ends.

$$b > |u_i(X) - u_i(\tilde{X})|.$$

The only way b can come into existence is that some event happen by the switcher's action to generate b. If nothing happens by the switch, that is, if the preference of the members of $S(x)$ is the preference of the members of $T(x)$, then no change occurs and b is not generated. Since no one gains, no one has a motive to compensate the switcher for switching. So, if the switcher is to be paid, the outcome must be changed, i.e., the switcher must be the pivot. Hence we have the second condition:

$$S(x) - \{i\} = -T(x) \tag{2}$$

Having defined switching, we now define vote-trading, which is a barter system of switching. A trade between $i \in N$ and $j \in N$ occurs when i switches on x and j switches on y in such a way that, though i loses from his switch, he gains from j's, and, though j loses from his switch, he gains from i's. The conditions for a trade are now specified.

Let i, $i \in S(x)$, order $XP_i\tilde{X}$. Let i switch (i.e., $-S(x) + \{i\} = T(x)$), so that the members of $T(x)$ bring about X as the chosen state of affairs. Of course, i loses something thereby; and, in order to compensate i, someone else must gain. Let the gainer be j, $j \in -S(x)$, so that $\tilde{X}P_jX$. By reason of i's switch, j has obtained his preferred state of affairs despite his bleak initial expectations. Thus, i has a loss, j has a gain, and j in a sense owes i a debt.

If this were a system of exchange in which participants paid for services with some numeraire, like money or jobs, then j might simply pay i at least the amount b. But since in fact it is a system of barter, j must pay i with a switched vote. For j to switch on motion y, it must be by (1) that j belongs to $S(y)$. Let j order $YP_j\tilde{Y}$ so that by his switch he brings about $\tilde{Y}$, which is what is desired by i, $i \in -S(y)$, who orders $\tilde{Y}P_iY$. Thereby, i is compensated for his previous loss and j has discharged his debt on motion x by taking a loss on motion y. Summarizing, the first condition of vote trading then is:

$$\begin{aligned} i &\in S(x), & j &\in -S(x) \\ i &\in -S(y), & j &\in S(y) \end{aligned} \tag{3}$$

Simple as this condition is, it tells us quite a bit about the actual occurrence of logrolling in real legislatures. It is apparent that, for a trade to be possible, the traders must be on opposite sides on two motions. One trader must be in the initial majority on the first motion and the initial minority on the second. The other trader must be in the initial minority on the first motion and the initial majority on the second. If this condition cannot be met, then trading is not possible.

One obvious place it cannot be met is in legislatures with just two disciplined parties. To say a party is disciplined is to say that all its members are on the same side on any motion. Hence if the members of a party are a majority on one motion, they are a majority on all motions. For the condition to be met, some of those people must sometimes be in the minority, but they cannot by reason of the fact of discipline.

Consequently, in a legislature with just two disciplined parties, logrolling is extremely rare. This is probably one reason so many American writers from Woodrow Wilson onward have admired the British Parliament, especially when it has approached the two-party situation in which statement (3) cannot be satisfied. They should have also admired those state legislatures like New York and Pennsylvania which party bosses have usually run with an iron hand because logrolling is equally impossible in them. But it has never been fashionable to admire boss rule.

Of course, when vote trading is banished from the legislature, political compromise goes on someplace else politically antecedent to the legislature. Thus in state legislatures and city councils with disciplined parties, it is in the majority caucus or in the mind of the boss that the compromise takes place. In England, the Cabinet serves as one place of compromise and very probably something like vote trading goes on there. Since the Cabinet situation is unstructured in comparison with the Parliamentary situation, however, it is probably hard to identify the trades and compromises that do occur.

To approach the second condition for vote trading observe that, in the running example where i, $i \in S(x)$, $i \in -S(y)$, orders $XP_i\tilde{X}$ and $\tilde{Y}P_iY$ while j, $j \in -S(x)$, $j \in S(y)$, orders $\tilde{X}P_jX$ and $YP_j\tilde{Y}$, it is the case that i gives up winning on x in order to win on y. Assuming that the trader is rational in the sense that he seeks to maximize utility, then he gives up on x only for a utilitarian reason. It must be, therefore, that, if he trades, the trader finds the joint utility of losing on x and winning on y greater than the joint utility of winning on x and losing on y.

To express this a little more formally, one can write of the utility of a trader's preferred position on motion x, $u_i(P_x)$, and of his not preferred position, $u_i(\tilde{P}_x)$. By statement (3) a pair of traders initially, without trading, obtain:

$$\text{for } i,\ u_i(P_x) + u_i(\tilde{P}_y)$$
$$\text{for } j,\ u_j(\tilde{P}_x) + u_j(P_y),$$

where i initially belongs to $S(x)$ and $-S(y)$ and j initially belongs to $-S(x)$ and $S(y)$. By trading, however, they can obtain:

$$\text{for } i,\ u_i(\tilde{P}_x) + u_i(P_y)$$
$$\text{for } j,\ u_j(P_x) + u_j(\tilde{P}_y).$$

For the trade to occur, it must be that the latter are, respectively, greater than the former:

$$u_i(\tilde{P}_x) + u_i(P_y) > u_i(P_x) + u_i(\tilde{P}_y)$$
$$u_j(P_x) + u_j(\tilde{P}_y) > u_j(\tilde{P}_x) + u_j(P_y)$$

or, rearranging:

$$\begin{aligned} u_i(P_y) - u_i(\tilde{P}_y) &> u_i(P_x) - u_i(\tilde{P}_x) \\ u_j(P_x) - u_j(\tilde{P}_x) &> u_j(P_y) - u_j(\tilde{P}_j). \end{aligned} \tag{4}$$

If we define the salience, s, to a member, i, of a motion, x, as the absolute difference between his utility for his preferred position and the utility for his not preferred position, that is,

$$s_i(x) = \left| u_i(P_x) - u_i(\tilde{P}_x) \right|$$

then we can express statement (4) easily in words: for a trader to trade, his salience must be higher on the motion on which his partner switches than it is on the motion on which he switches.

In Tables 1 and 2 is presented an instance of a three-member committee in which vote trading is possible between members 2 and 3. In that example, the arrangements in the first column are made to satisfy statement (3) that member 2 belongs to $S(x)$ and $-S(y)$ and member 3 belongs to $-S(x)$ and $S(y)$. Ignoring for the moment member 1's utilities (except as his preference is necessary to provide an initial majority), we construct the tables with cardinal utility numbers satisfying both on the one hand a set of preferences ($XP_2\tilde{X}$, $\tilde{Y}P_2Y$, $\tilde{X}P_3X$, $YP_3\tilde{Y}$) and on the other hand the utility requirements of statement (4). Translating statement (4) into the example of Tables 1 and 2, one gets

$$\begin{array}{ccc} u_2(\tilde{Y}) - u_2(Y) & > & u_2(X) - u_2(\tilde{X}) \\ 3-(-3) & & 1-(-1) \end{array}$$

$$\begin{array}{ccc} u_3(\tilde{X}) - u_3(X) & > & u_3(Y) - u_3(\tilde{Y}). \\ 2-(-4) & & 1-(0) \end{array}$$

For comparison, in Tables 3 and 4 is presented an example that satisfies statement (3) but not statement (4). Again translating, one gets

$$\begin{array}{ccc} u_2(\tilde{Z}) - u_2(Z) & = & u_2(W) - u_2(\tilde{W}) \\ 1-(-1) & & 1-(-1) \end{array}$$

$$\begin{array}{ccc} u_3(\tilde{W}) - u_3(W) & = & u_3(Z) - u_3(\tilde{Z}). \\ 1-(-1) & & 1-(-1) \end{array}$$

And because these are equalities rather than inequalities, statement (4) is not satisfied. Presumably, 2 and 3 could not trade on motions w and z.

From these examples one can readily see the advantages of vote trading and easily understand why the notion of logrolling has been so attractive to scholars. There may be a clear gain from trade for both parties. In the example in Tables 1 and 2, member 2 gains four units of utility and member 3 gains five units.

Nevertheless there are some severe limitations. The possibilities of trading are quite restrictedly

Table 1

Motion x

	Preferred Outcome	$u(X)$	$u(\tilde{X})$
Member 1	X	—	—
Member 2	X	1	−1
Member 3	$\tilde{X}$	−4	2

Table 2

Motion y

	Preferred Outcome	$u(Y)$	$u(\tilde{Y})$
Member 1	Y	—	—
Member 2	$\tilde{Y}$	−3	3
Member 3	Y	1	0

Table 3

Motion w

	Preferred Outcome	$u(W)$	$u(\tilde{W})$
Member 1	W	—	—
Member 2	W	1	−1
Member 3	$\tilde{W}$	−1	1

Table 4

Motion z

	Preferred Outcome	$u(Z)$	$u(\tilde{Z})$
Member 1	Z	—	—
Member 2	$\tilde{Z}$	−1	1
Member 3	Z	1	−1

finite. By reason of statement (2), a switch is pointless unless the switcher is a pivot with respect to the initial distribution of preference. Consequently, we have a maximum limit on logrolling: Since it takes two pivotal members to trade, there can at most be as many trades as half the number of issues. In a temporal world, moreover, only a finite number of issues can be juggled at once. In a session of a typical national legislature, there are probably no more than, say, two thousand motions, which gives a theoretical maximum of one thousand trades. But of course, many issues are trivial so that no trade is worthwhile; many others come up for decision so quickly that the elaborate arrangements of logrolling are not possible; for many others it may be difficult for a member to discover another one with whom statement (4) can be satisfied; and, finally, since motions come up serially so that many future motions cannot be anticipated and so that past motions are irrevocably settled, only a small subset of motions is available for trading at any one moment. One suspects that the practical maximum of trades is at most one-tenth of the theoretical maximum. Consequently, in the typical legislature (with, say, one hundred members) in the typical session it may well be that the average member can expect to be involved in at most one trade.

Furthermore, while some members may get enormous gains from trade, such enormous gains may be rare. In this sense vote trading, subject as it is to severe temporal restrictions, is quite different from commodity trading which can go on forever. So while it is true that gains can be made from trade, it is doubtful that the size and frequency of such gains is as great as the exponents of logrolling have imagined. Nevertheless, so long as one considers only the utilities of the traders themselves, trading is, for certain, both individually and socially desirable.

The Paradox of Vote Trading

Unfortunately, even this moderated enthusiasm for logrolling may be misplaced because the admitted gains from trade may be wiped out by external costs. Indeed, it is quite possible to imagine a system in which everyone gains from individual trades—and so has a positive motive to logroll—and yet everyone also loses if all such trades are carried out. This is the paradox of logrolling, to the illustration of which we now turn.

External costs are those which one bears as a result of the actions of others. They are to be contrasted with internal costs which result from one's own action. The noise that comes from the pneumatic drill is an internal cost with respect to the driller because it is part of what he must suffer to earn his pay. But to the neighbor at the construction site, the noise is an external cost. He must bear it not in order to get something else, but merely because he is there.

Debate is never ending about the existence of some external costs. (For example, does the noise of the drill really bother the neighbor, or does he see a chance, by complaining of noise, even though it does not bother him, to blackmail the contractor?) But it seems impossible to doubt the existence of external costs in vote trading. Consider the position of some member who is not a party to a trade: Since the trade brings about a different winner on a motion, the nontrader bears an external cost if he was originally in the majority or he receives an external gain if he was in the original minority. Thus, if members i and j trade on motions x and y, then if some other member k belongs to $S(x)$ and $S(y)$, k is forced by their action to belong to $-T(x)$ and $-T(y)$. Thus k suffers a loss of $|u(P_x)-u(\tilde{P}_x)|+|u(P_y)-u(\tilde{P}_y)|$—all as a consequence of no action of his own. Indeed, member k is completely passive throughout the entire transaction, but his outcomes are reversed.

External costs have generally been ignored in the writings of those who extol the benefits of vote trading.[12] Yet they must by the nature of the case, i.e., by statement (2) on pivoting, be present. Since trading changes the outcomes, there must always be innocent bystanders who lose. Although the size of their losses depends on the salience of particular outcomes in particular cases, it is possible to show that in some particular cases suffering is general and universal, which is the paradox of logrolling.

Table 5 depicts a system of trades for a three member legislature: between members 2 and 3 on motions x and y, between members 1 and 2 on motions w and z, and between members 1 and 3 on motions t and v. (This is not the only possible set of trades—one might have members 2 and 3 trade on x and w, 1 and 2 on t and y, and 1 and 3 on v and z, and other less exhaustive combinations—but all other complete sets of trades share the paradoxical feature displayed for the trades in Table 5. We assume, however, that once a set of trades on particular issues is made, new trades cannot be made on these issues. This means that a trader may not cancel an old trade with a new one simply because he subsequently discovers that what was thought to be advantageous is no longer

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[12] An exception is Coleman, p. 1121, where it is noted in passing that the inclusion of such costs requires a "much more extensive calculation" than offered in that essay. More recently Coleman has provided an explicit calculation that leads to the conclusion that vote trading is profitable only under conditions of "absolutely free and frictionless political exchange" that preclude arbitrary decision rules. James S. Coleman, "Beyond Pareto Optimality," in *Philosophy, Science, and Method,* ed. Sidney Morgenbesser, Patrick Suppes, and Morton White (New York, St. Martin's, 1969), pp. 415–39.

Table 5. Utilities of Members for Outcome of Voting

	Preferred Outcome Col 1	Motion *x* $u_i(X)$ Col 2	$u_i(\tilde{X})$ Col 3	Preferred Outcome Col 4	Motion *y* $u_i(Y)$ Col 5	$u_i(\tilde{Y})$ Col 6
Member 1*	X	1	−2	Y	1	−2
Member 2	X	1	−1	$\tilde{Y}$	−2	2
Member 3	$\tilde{X}$	−2	2	Y	1	−1
		Motion *w* $u_i(W)$	$u_i(\tilde{W})$		Motion *z* $u_i(Z)$	$U_i(\tilde{Z})$
Member 1	W	1	−1	$\tilde{Z}$	−2	2
Member 2	$\tilde{W}$	−2	2	Z	1	−1
Member 3*	W	1	−2	Z	1	−2
		Motion *t* $u_i(T)$	$u_i(\tilde{T})$		Motion *v* $u_i(V)$	$u_i(\tilde{V})$
Member 1	$\tilde{T}$	−2	2	V	1	−1
Member 2*	T	1	−2	V	1	−2
Member 3	T	1	−1	$\tilde{V}$	−2	2

* Nontrader on pair of motions.

so. Such an assumption is realistic because most legislatures provide ways to render past votes irrevocable, which means completed or partially completed bargains cannot be undone. In the next section, however, we drop even this assumption and show that the paradox persists even with unrestricted trading.) We have worked out examples similar to those of Table 5 for a five-member legislature, but they are too lengthy for inclusion here. Doubtless similar examples could be worked out for legislatures of any size. The three-member example can, of course, be embedded in any legislature; and, of course, the three members can be three factions.

It is quickly apparent that, if no trading occurs, the members neither gain nor lose from the passage of the six motions imagined for Table 5. The calculation is set forth in Table 6, where the several columns are a simple reproduction of columns 3 and 5 of Table 5—since in that Table all motions pass under sincere or naive voting. In Table 6 in the summary column on the extreme right, each member earns a net utility of zero as a result of the six votes.

Suppose, now, that the members, starting from this base point of zero, calculate what they may expect to make from the trades in which they can engage. Each member sees that he can make two trades and increase his utility thereby. For the third pair of motions he can do nothing and might reasonably expect, though his expectation is in fact unrealizable, that he will receive his nontrading outcome on these motions. His expectations are, therefore, set forth in Table 7. If the member so calculates, it is clearly advantageous to trade whenever possible. When each does so, each generates a clear gain of four units of utility. From the point of view of the member, by taking these actions he increases his expected utility—which is the essence of rational behavior.

Unfortunately, however, if each member behaves rationally by making the trades possible for him, all the members suffer. They are, in fact, *worse* off than if they had voted sincerely or naively. The calculation is set forth in Table 8, where it is shown that each member receives a negative two units when all trades are made and external costs are taken into the reckoning.

This is the paradox of logrolling: that rational trades by all members make everyone worse off.

Table 6. Utilities of Outcomes Without Trades (Sincere Voting)

Members	Outcomes $u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	Σu_i
1	1	1	1	−2	−2	1	0
2	1	−2	−2	1	1	1	0
3	−2	1	1	1	1	−2	0

Table 7. Utilities of Outcomes with Trades When Possible and without Anticipating External Costs

	Outcomes						
Members	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	Σu_i
1	1	1	−1	2	2	−1	4
2	−1	2	2	−1	1	1	4
3	2	−1	1	1	−1	2	4

Table 8. Utilities of Outcomes with Trades and with External Costs

	Outcomes						
Members	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	Σu_i
1	−2	−2	−1	2	2	−1	−2
2	−1	2	2	−1	−2	−2	−2
3	2	−1	−2	−2	−1	2	−2

It is not easy to extricate oneself from this paradox. For example, it might be said that if members were foresighted they would refuse to make trades at all, lest the trades lead to the realization of the paradox. Self-restraint is not so easy to achieve, however. Suppose each member makes no trade himself but anticipates a trade among the pair that does not include him. That is, he himself behaves with self-restraint; but, with a conservative appreciation of what others can do to harm him, he anticipates the worst from them. This expectation, which is analogous to the notion of a security level in the theory of games, is calculated for each of the members in Table 9. It is immediately apparent that this is the worst that can happen to any member, for each loses six units of utility.

If each member perceives that his security level (Table 9) without trading is −6, then the −2 from systematic trading (Table 8) looks pretty attractive. It looks even more attractive if one suspects for a moment that the members of the pair from which one is excluded will in fact trade. Then one's only protection is to trade oneself as much as possible. This then guarantees a minimum loss of −2.

Conceivably the members might escape the paradox by agreeing to vote sincerely. But, if any member suspects that others may not abide by their promises, then the best strategy is to trade oneself. In this way it is rational to trade not only to try to achieve the maximally good results (of Table 7) but also to avoid the minimal results (of Table 9) by arriving at modest losses (in Table 8). Thus, each member must trade in order to behave rationally; but then, perversely, each member comes to a worse end than if he abstained from trades and always voted sincerely or naively. The paradox is inescapable—in fact it is a kind of *n*-person Prisoners' Dilemma as depicted in Table 10.

Some Conditions of the Paradox

This paradoxical result is not an accidental consequence of the particular cardinal utilities nor of the particular choices of trades to be made. Rather the paradox persists even if one uses ordinal utility and allows all possible trades. To show this we now translate our discussion into ordinal utility and develop an appropriate example.

In the example of Table 11, the six motions now no longer have cardinal utilities associated with them. Instead, reading across each row are the six initial majority (i.e., "$S(x)$") and minority (i.e., "$-S(x)$") sets to which each member belongs

Table 9. Security Levels when No Trades Occur but Each Member Anticipates External Costs

	Outcomes						
Members	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	Σu_i
1	−2	−2	1	−2	−2	1	−6
2	1	−2	−2	1	−2	−2	−6
3	−2	1	−2	−2	1	−2	−6

Table 10.

The Paradox of Vote Trading as a Three-Person Prisoners' Dilemma

Member 3		Member 2 Alternative 1 Not Trade	 Alternative 2 Trade
Alternative 1 Not Trade			
Member 1	Alternative 1 Not Trade	0, 0, 0	0, 0, 0
	Alternative 2 Trade	0, 0, 0	2, 2, −6

Member 3		Member 2 Alternative 1 Not Trade	 Alternative 2 Trade
Alternative 2 Trade			
Member 1	Alternative 1 Not Trade	0, 0, 0	−6, 2, 2
	Alternative 2 Trade	2, −6, 2	−2, −2, −2

Note: In cell (1, 1, 1) the entry is "0, 0, 0" because no one trades. In cells (1, 2, 1), (2, 1, 1), and (1, 1, 2) the entries are also "0, 0, 0" because, if any pair consistently vote sincerely, then all three must do so. In cells (2, 2, 1), (1, 2, 2), and (2, 1, 2) the entries are appropriate combinations of 2 units for the traders and −6 units for the non-trader. In these cases, the −6 units are as calculated in Table 9, while the 2 units result from receiving, first, the payoff to sincere voting (a) when one is not a trader (1+1) and (b) when one's potential trade is with a sincere voter (−2+1); and second, the payoff to sophisticated voting when one's partner is a sophisticated voter (−1+2). In cell (2, 2, 2), the entry (−2, −2, −2) derives from Table 8. This is strictly a Prisoners' Dilemma because, while the outcome (in cell (1, 1, 1)) from the strategy of not trading is preferred by all three players to the outcome (in cell (2, 2, 2)) from the strategy of trading, still the strategy of trading dominates for all three players and the not preferred outcome must inevitably occur among rational players of a non-cooperative game.

when he expresses his true tastes. The sets are arranged from left to right according to the salience (i.e., $s_i(x) = |u_i(P_x) - u_k(\tilde{P}_x)|$) of the motion to the member, with the most salient motion on the left and the least salient motion on the right. For member 1, for example, victory for his position on *x* is ordinally most valuable, victory on *y* second most valuable, . . . , and victory on *v* least valuable. That is, $s_1(x) > s_1(y) > \cdots > s_1(v)$. Recalling the second condition of vote trading (statement (4) that "for a trader to trade his salience must be higher on the motion on which his partner switches than it is on the motion on which he switches"), one can easily find trades for each pair of members. Thus member 1 can trade with member 3 on motions *t* and *v*. That is, member 1 votes against his interest on his least salient motion, *v*, and gains thereby from member 3's switch on 1's third most salient motion, *t*. Similarly, member 3 votes against his interest on his next to least salient motion, *t*, and gains thereby on his motion of next higher salience, *v*. These trades are indicated in Table 11 by lines connecting the positions of a member for each trade in which he may be engaged.

After the trades indicated in Table 11 have been made, it can be rewritten as Table 12 in terms of the resultant majorities (i.e., "$T(x)$") and minorities (i.e., "$-T(x)$") where "$T_d(x)$" means that the member is on the winning side only because he has voted sophisticatedly and in spite of the fact that he disagrees ("*d*") with it.[13]

It is easy to see that this is a paradoxical result. Each member loses a majority position on his two most salient motions, and incidentally obtains a majority position contrary to his true tastes on his least salient motions. For thus losing on his two most and his two least salient motions, he gains

Table 11.

Members	High Salience Majority and Minority Positions of Members before Trades					Low Salience	Trades	Switches	
1	$S(x)$	$S(y)$	$-S(t)$	$-S(z)$	$S(w)$	$S(v)$	with 3	to $T_d(v)$	——
							with 2	to $T_d(w)$	....
2	$S(t)$	$S(v)$	$-S(w)$	$-S(y)$	$S(x)$	$S(z)$	with 1	to $T_d(z)$	....
							with 3	to $T_d(x)$	oooo
3	$S(w)$	$S(z)$	$-S(x)$	$-S(v)$	$S(t)$	$S(y)$	with 2	to $T_d(y)$	oooo
							with 1	to $T_d(t)$	——

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[13] We assume that the utility of a member for a majority position on a motion he disagrees with, ($T_d(x)$), is the same for him as the utility of a minority position on that same motion, ($-T(x)$), because in both cases his sincere position differs from the prevailing one.

Table 12.

Member	High Salience Majority and Minority Positions after Trades					Low Salience
1	$-T(x)$	$-T(y)$	$T(t)$	$T(z)$	$T_d(w)$	$T_d(v)$
2	$-T(t)$	$-T(v)$	$T(w)$	$T(y)$	$T_d(x)$	$T_d(z)$
3	$-T(w)$	$-T(z)$	$T(x)$	$T(v)$	$T_d(t)$	$T_d(y)$

only on his mediumly salient motions. Thus, clearly, since the degree of utility where he worsens his position is greater than the degree of utility where he betters his position, he is in sum hurt by the set of trades.

It might be thought that this result could be avoided by a different set of initial trades. But, referring back to Table 11, it is apparent that such trades are not possible. Member 1, for example, wants to gain on motions t and z by getting someone to switch on these motions. He cannot trade with member 2 on motion t, however, because this would force 2 to switch on his most salient motion in clear violation of statement (4). Therefore, member 1 *must* trade with member 3 on motion t as in Table 11. Similarly member 1 cannot trade with member 3 on motion z because this would force 3 to vote against his true tastes on a more salient motion (z) in order to gain his taste on a less salient one (x or v). Again, therefore, member 1 *must* trade with member 2 on motion z, exactly as in Table 11. By a similar argument, each of the members in this Table is constrained to exactly those trades included in it.

The example in Table 11 shows that the paradox is inherent in the ordinal relationships and is not dependent on particular cardinal utility numbers. It is not necessarily true, however, that the situation depicted in Table 12 is in equilibrium. If one translates the post trading agreements of Table 12 into a new round of trading one gets Table 13, which, if the indicated trades are made, takes the three members exactly where they were in the beginning in Table 11. Thus by voting sophistiactedly in the first round they get to a paradox from which they can extricate themselves by voting sincerely in the second round.

Voting that involves first a switch to a position contrary to one's true tastes and then a reverse switch back may seem more devious than sincere. But this is only the beginning. For after a reverse shift, when everybody assumes once again his original sincere positions, there is no reason to believe that pairs of members will not plunge themselves once again into a new round of trades which are individually rational. Indeed, the inexorable logic of individual rationality dictates that if a member is willing to doublecross an old trading partner in order to improve his position through another trade, he is willing to triplecross his new trading partner, etc. The consequence of all members' acting in this self-serving fashion is that trading continues indefinitely. The lack of a stable equilibrium that could break this cycle means that there is no guarantee that even the improved benefits to all, as before the initial or after the subsequent trades in our example, will halt the process. In practice, only when motions are actually voted upon will the process of vote trading be terminated.

Is there any way in which this cyclical paradox can be avoided? One member might refuse to trade; but, if he does so, he exposes himself (as we showed in the previous section) to external costs from the other pair's trade without any compensating gains from his own trade. Hence, as we previously noted, it is a kind of n-person Prisoners' Dilemma in which each member is forced by individual rationality to choose the socially and individually destructive alternative.

Another possible way to avoid the paradox is for a pair to form a coalition agreeing to trade only with each other. Thereby they might obtain the benefits of trade and avoid the external costs of others' trades. This solution was suggested by Dr. Edwin T. Haefle in a critique of the original version of this essay when it was presented at the

Table 13.

Member	High Salience Majority and Minority Positions before Round 2 of Trades					Low Salience	Trades	Switches	
1	$-S(x)$	$-S(y)$	$S(t)$	$S(z)$	$S_d(w)$	$S_d(v)$	with 2	to $T(v)$	——
							with 3	to $T(w)$	----
2	$-S(t)$	$-S(v)$	$S(w)$	$S(y)$	$S_d(x)$	$S_d(z)$	with 3	to $T(z)$	oooo
							with 1	to $T(x)$	——
3	$-S(w)$	$-S(z)$	$S(x)$	$S(v)$	$S_d(t)$	$S_d(y)$	with 1	to $T(y)$	----
							with 2	to $T(t)$	oooo

1972 Annual Meeting of the American Political Science Association.[14] Of course, this solution simply denies statement (3), the first condition for vote trading; but it is one way to cut the Gordian knot. It is comparable to cooperative solutions to the Prisoners' Dilemma where, with preplay communication allowed and criminals free to make binding agreements, they agree never to confess and thereby thwart the prosecutor. In the absence of enforceable agreements, however, there may be instabilities in such solutions simply because they are not individually rational. So it is here.

Referring again to Table 11, suppose members 2 and 3 coalesce on motions x and y. Member 1 is excluded, but he may propose that he and member 2 coalesce instead on motions z and w. Once this alternative is proposed, member 2 prefers the coalition with member 1 on z and w to the coalition with member 3 on x and y simply because he (2) gains more from an improvement of his position on his more salient motion, w, than he does from an improvement on his less salient motion, y, and he loses less from voting sophisticatedly on his least salient motion, z, than from so voting on his next-to-least salient motion, x. Thus member 2 prefers coalition (1, 2) to coalition (2, 3) and, of course, member 1—who is in the former but not the latter—also prefers coalition (1, 2) to coalition (2, 3). One can write then

$$(1, 2)\ D\ (2, 3)$$

where "D" means that the former coalition dominates the latter in that both members of the former prefer it to the latter.

By a similar argument it can easily be shown that coalition (2, 3) dominates coalition (1, 3). Suppose coalition (1, 3) forms over t and v. Then coalition (2, 3) involving x and y is preferred by member 3 because he (3) gains in coalition (2, 3) on x which is more salient for him than v and loses in coalition (2, 3) on y, which is less salient for him than t. Hence:

$$(2, 3)\ D\ (1, 3)$$

Finally, coalition (1, 3) which involves t and v is preferred to coalition (1, 2) which involves z and w, simply because t is of higher salience to member 1 than is z and v is of lower salience to him than is w. Hence:

$$(1, 3)\ D\ (1, 2)$$

Putting these statements together we obtain an intransitive result:

$$(1, 2)\ D\ (2, 3)\ D\ (1, 3)\ D\ (1, 2).$$

As is well known, when domination is intransitive the system is unstable. There is always one member left out of the coalition and this member has a strong incentive to break up any existing coalition. Furthermore, he can always do so because it is always possible for him to offer an alternative coalition that dominates an existing one.

This fact suggests that the paradox of vote trading is closely related to the paradox of voting. In Table 14 we have stated the preferences of each member for the possible trades that might be made. Thus for member 1, the highest preference is for the trade on motions t and v with member 3 simply because t is the most salient motion on which 1 can improve his position and v is the least salient motion on which he can vote sophisticatedly. His medium preference is for the trade on motions z and w (with member 2) because z is the next most salient motion on which 1 can improve his position and w is the next-least-salient motion on which he can vote sophisticatedly. His lowest preference is for the trade on motions x and y (between members 2 and 3) on which he is left out. If, now, the members vote on which trades they wish to occur, the outcome will of course be an intransitive social arrangement:

Table 14.

Member	Preference for Trades		
	Highest Preference	Medium Preference	Lowest Preference
1	(t, v)	(w, z)	(x, y)
2	(w, z)	(x, y)	(t, v)
3	(x, y)	(t, v)	(w, z)

(t, v) is socially preferred to (w, z)
by members 1 and 3,
(w, z) is socially preferred to (x, y)
by members 1 and 2,

and

(x, y) is socially preferred to (t, v)
by members 2 and 3

so that the arrangement is a cycle:

$$(t, v) \rightarrow (w, z) \rightarrow (x, y) \rightarrow (t, v),$$

where the arrow indicates a majority preference.

Thus, no trade is preferred to all other trades, which is equivalent to our earlier demonstration that no coalition can defeat all others. Since there is no preferred set of trades on which to base a stable coalition, no stable coalition is possible. The paradox of vote trading cannot, therefore, be simply solved by waving it away with a coalition. Rather it is inherent in the nature of the legislative process and, given an appropriate distribution of tastes and external costs, cannot be avoided.

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[14] Edwin T. Haefle, "Paradox Lost" (Washington D.C., mineographed, 1972).

This absence of stability in coalitions means that vote trading cannot assure a dominant collective preference.[15] Just as a cycle among all members (as in Table 13) precludes a dominant collective preference, so the instability of coalitions precludes a dominant collective preference among proper subsets of members. There seems, therefore, to be an inherent disequilibrium in vote trading which is occasioned by the assumption of individual rationality.

The Significance of the Paradox

The popular distrust of logrolling derives, we suspect, from an intuitive, incomplete, but, nonetheless, sure comprehension of this paradox or at least of the external costs on which it is based. To make a judgment on the validity of the popular distrust requires, therefore, that one estimate the likelihood that the paradox arise in real legislatures. If it arises frequently, then surely the popular distrust has a reasonable basis. If it arises only rarely, however, then one probably ought to conclude that logrolling is a socially useful technique to approach Pareto-optimal states of the world. (The positions of members are Pareto-optimal if there are no trades such that the position of at least one member can be improved without detriment to any other members.)

There is no systematic way, so far as we can discover, to investigate real world frequencies of the paradox. One can, however, specify types of circumstances in which it is likely to occur and then estimate the likelihood of these circumstances.

The crucial element of the paradox is external costs, which are of course unique to each trade. It is always the case, however, that more people gain than lose on each switch (i.e., the members of $-S(x)$ and the pivotal member(s) of $S(x)$ gain, while the non-pivotal member(s) of $S(x)$ lose.) Consequently, in a system of trades—and usually for each participant as well—more instances of gain occur than do instances of loss. But gains and losses are not necessarily equal in size so the paradox occurs when, in a system of trades, members generally lose more when they lose than they gain when they gain. The question then is: Under what real world circumstances might these large losses be expected to occur?

Manifestly, if there is only one trade, the paradox is impossible because the gainers on that trade cannot suffer external costs on another, which does not exist. Consequently, if trades are occasional and isolated and not part of a system of interrelated bargains, losses are probably less than gains for many members of the legislature. This kind of sporadic logrolling is often found in American legislatures, but it is not the kind so earnestly recommended by the several scholars cited in the beginning of this essay. The kind they recommend is, rather, that in which members engage in a constant round of bargaining in order to arrive at optimal outcomes over the whole set of issues in the legislative session. Unfortunately, it is exactly this kind that produces the possibility of the paradox.

If one can separate out his reformist concerns, this is what Schattschneider seems to have perceived about the writing of the Smoot-Hawley Tariff. Each member who joined the tariff combination for a price on some issue (usually a tariff on something manufactured in his district) gained an advantage for himself and his constituents. Such a large number joined, however, that protection was made indiscriminate—nearly everybody got something. Thereby international trade was discouraged and disrupted—to the disadvantage of everybody in the society. To the extent that the Smoot-Hawley tariff deepened and extended the Great Depression, even the gainers probably suffered more than they gained.

One can see the same process at work in, for example, the Rivers and Harbors bills of the twentieth century when they had ceased to have the character of vital internal improvements that they had in the nineteenth. A majority gain from the pork in such barrels, else the bills would not pass. But there are also losses, external costs. Once taxes are paid, the net gains of the gainers are probably pretty small. And if the projects are for the most part economically useless (like locks on a canal only a few pleasure boats use), then opportunity costs probably exceed the gainers' gains. And if the projects are positively harmful (like dikes on a river that would otherwise support wet lands and a high water table), then their absolute losses almost certainly exceed the gainers' gains.

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[15] For the case of cardinal utility, R. E. Park has shown that there is in general no stable equilibrium if there is a majority that can, through vote trading, improve the payoffs to all its members over the payoffs without vote trading. Furthermore, he has shown that if there is a stable equilibrium with vote trading, it must be exactly the same as the outcome without vote trading. R. E. Park, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, vol. 57 (December 1967), 1301–04. In "The Possibility of a Social Welfare Function: Reply," *ibid.*, pp. 1311–17, however, James Coleman argues that Park's assumptions are unrealistically restrictive.

Still other possible cases come readily to mind: As a long-term example, there is the system of income tax exemptions and deductions, which are so generally distributed they actually provide savings for only an unidentifiable few and which at the same time probably assess very high costs against nearly everybody in the form of distortions of the market for many goods. For another—and even longer term—example, there is the proliferation of army bases throughout the country, each providing a small economic benefit to its neighborhood, but by their inefficiency considerably increasing in sum the military cost for everybody.

As these examples suggest, the bills and circumstances likely to occasion paradoxes are those which bring together an interrelated set of issues, many of which are of interest to only a few legislators. Tax bills, internal improvement bills, redistributions of income, etc. seem especially prone to providing the kind of situation in which the paradox can easily arise. Since these kinds of bills and business occupy a good part of any legislature's time, it must be concluded that the paradox of vote trading is a real fact of political life. One suspects that, because of it, logrolling leads more often away from than toward Pareto-optimal social allocations.

With a bow in the direction of reformers, both those who recommend and those who excoriate logrolling, it might be asked whether or not occurrences of the paradox can be prevented. The answer is, probably not, short of highly unpalatable restrictions. Legislatures do occasionally adopt self-denying ordinances, like the so-called closed rule in the House of Representatives, a rule that prohibits amendments from the floor. While this probably prevents the grosser and more thoughtless types of logrolling, the anticipation of it probably forces the trading back off the floor into committee. Given all the rational pressures for trading, it is not really to be expected that a policy of self-denial will work. Stronger measures, like the system of responsible and disciplined parties, may well make trading impossible in the legislature, but they certainly cannot prevent it in the executive branch or in the party caucus.

It seems, then, that so long as we have the open kind of legislature in which any pair of members may trade, then trading cannot be eradicated. Nevertheless, speaking normatively, the paradoxical consequences are unpalatable and one probably ought to try—by such devices as closed rules and popular condemnation—to discourage the kinds of logrolling that generate paradoxes.

Schema Theory: An Information Processing Model of Perception and Cognition*

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I. Introduction

A. *The Idea of a Schema*

The world is complex, and yet people are able to make some sense out of it. A national or international political arena, for example, is so huge and so complex that to make any sense out of it seems to be a superhuman task. And yet national leaders and even the man in the street do make more or less intelligent interpretations about political events and relationships. How do they do this?

One of the most important tools that people use is a schema. A formal definition will be given later, but informally a schema is a "preexisting assumption about the way the world is organized."[1] When new information becomes available, a person tries to fit the new information into the pattern which he has used in the past to interpret information about the same situation. If the new information does not fit very well, something has to give.

Balance is an important example of a commonly used schema, and its use can be illustrated by friendship relationships. A person has a balanced set of beliefs about his acquaintances if he believes that all of his friends like each other, all of his enemies also like each other, and each of his friends dislikes and is disliked by each of his enemies. Thus a balanced set of friendship relationships provides a tidy view of the world in "black and white" terms. It implies that friends of friends are friends, enemies of friends are enemies, and enemies of enemies are friends. More formally, balance can be defined in terms of a set of objects and a set of relationships between them. The whole collection is balanced if the objects can be classified into no more than two nonoverlapping sets (e.g., "friends" and "enemies") so that all the relationships which exist between members of the same set are positive (such as "likes") and all the relationships which exist between members of differing sets are negative (such as "dislikes").

Suppose a person interprets the relationships between his acquaintances as fitting a balanced schema. Then if he receives new information about a particular relationship or about several relationships he can try to fit this new information into the old interpretation. If the new information fits exactly into his previous expectations, there is no problem. If it does not fit exactly, he has a variety of tactical choices. He can try to use the same schema as before with the same interpretation of who are his friends and who are his enemies. This will require discounting the new discrepant information. He might also use the same schema of balance, but respecify which people are his friends and which are his enemies. Or he may use another schema entirely, and interpret the information as indicating (for example) that there are three different clusters of people (rather than two)

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In reviewing the psychological literature I have relied heavily on Gardner Lindzey and Elliot Aronson, eds., *The Handbook of Social Psychology*, second edition (Reading, Mass.: Addison-Wesley, 1968, 1969), especially the chapters by Berger and Lambert "Stimulus-Response Theory in Contemporary Social Psychology," I, 81–178; Seymour Rosenberg, "Mathematical Models of Social Behavior," I, 179–244; Robert B. Zajonc, "Cognitive Theories in Social Psychology," I, 320–411; Leonard Berkowitz, "Social Motivation," III, 50–135; Henri Tajfel, "Social and Cultural Factors in Perception," III, 315–394; and George A. Miller and David McNeill, "Psycholinguistics," III, 666–794.

[1] Jerome E. Singer, "Consistency as a Stimulus Process Mechanism," in *Theories of Cognitive Consistency: A Sourcebook*, ed. Robert P. Abelson, Elliot Aronson, William McGuire, Theodore Newcomb, Milton Rosenberg and Percy Tannenbaum (Chicago: Rand McNally, 1968).

who all like other members of the same cluster and dislike anyone not in their cluster.[2]

This paper is written with three audiences in mind. For the general political scientist interested in the policy-making process, the paper integrates a variety of psychological research into an organizing framework which can be used as an introduction to a number of relevant subfields of psychology. For the political scientist who is specifically concerned with attitudes and beliefs or the role of perceptual and cognitive processes in politics, the paper is offered in the hope of providing new insights into how people can and should cope with the highly complex environments typical of political arenas. For the experimental and social psychologists whose work is being used, this paper offers an information-processing model of perception and cognition which I call "schema theory." This schema theory has theoretical implications that go beyond the particular findings used. For all readers, the schema theory is meant to raise new questions that can guide further empirical as well as theoretical work. In this way the inaccuracies and overgeneralizations of the current model can be discovered, alternatives proposed, and our knowledge advanced.

At the start it is best to have clearly in mind what the schema theory is about and what it is not about. It is about how a single person observes and makes sense out of a complex environment. Therefore it describes the perceptual and cognitive processes of a single person; it does *not* describe the functioning of a small group, let alone a social movement or a government. Furthermore, it describes how a person processes information and tries to make sense out of it, but it does not describe how he makes policy decisions. This paper is designed to stand alone, but it can also be regarded as part of a larger project which deals with groups as well as individuals and with decision making as well as information processing. The structure of such a larger project is suggested in the author's *Framework for a General Theory of Cognition and Choice.*[3] An application of part of that framework to the decision-making process in an actual governmental committee is given in the author's "Psycho-algebra: A Mathematical Theory of Cognition and Choice with an Application to the British Eastern Committee in 1918."[4] The present paper focuses on the information-processing aspects of the overall framework. It employs findings from experimental and social psychology to help formulate a schema theory which provides falsifiable substance to that part of the framework.

The schema theory itself takes the form of a partially specified model of how people process new information. Once the model is presented with the help of a flow-chart, a variety of findings from experimental psychology will be cited to support some of the decisions which went into the construction of this version of the model. Then further empirical findings will be cited to show that some of the predictions which the schema theory makes are valid at least in certain circumstances. The approach used here will then be compared to some other theoretical approaches, and consideration will be given to some of the outstanding research questions raised by the theory. Finally, a few illustrations from international relations and especially foreign policy formation show that this model of how people make sense out of a complex world can be directly relevant to the study of important political processes.

B. *The Elements of the Model*

The input to the model is a *message,* which contains several types of information. Each message has a known *source,* which may be one's own direct observation or another person. The heart of the message is some *information* about a particular case, such as the state of the friendship relationships between a certain group of individuals. The *type of case* (for example "friendship relationships") is also known. Thus a message in this model might take the form, "Mr. Smith says, 'Joe likes Bill, and Bill likes Sam.' " A full description of this particular case would indicate who likes whom and who dislikes whom among the whole set of relevant people, although the message itself need not contain a complete specification.

A *schema* in this model is defined as a subset of all the possible specifications of cases. A schema can be arbitrarily defined as any subset of the specifications, but typically a schema is defined in terms of the set of all specifications which have certain stipulated properties. For example, the schema of *balance* is defined as the collection of all specifications which divide objects into two nonoverlapping groups with positive relationships only within groups and negative relationships only between groups. A larger schema would be *symmetry*, in which the only requirement is that whatever A's relationship is to B, B has to have the same relationship to A. Obviously, any complete specification which is balanced is also symmetric, but not vice versa.

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[2] For a formal treatment of balance see Frank Harary, Robert Norman and Dorwin Cartwright, *Structural Models* (New York: Wiley, 1965). For a treatment of clusters see James Davis, "Clustering and Structural Balance in Graphs," *Human Relations,* 20 (May, 1967), 181–187; and James A. Riley, "An Application of Graph Theory to Social Psychology," in *The Many Facets of Graph Theory,* Lecture Notes in Mathematics, No. 110 (New York and Berlin: Springer Verlag, 1969).

[3] Robert Axelrod, *Framework for a General Theory of Cognition and Choice* (Berkeley: Institute of International Studies, University of California, 1972). Also to appear in *Theories of Collective Behavior,* ed. Julius Margolis and Henry Teune, forthcoming.

[4] Robert Axelrod, "Psycho-Algebra: A Mathematical Theory of Cognition and Choice with an Application to the British Eastern Committee in 1918," *Papers of the Peace Research Society (International),* 18 (1972), 113–131.

A *case* is a specific instance in time. A particular case might represent, for example, the friendship relationships among a given group of people at the present time. An *interpretation* of a case in terms of a schema is a treatment of the case as if it belonged to that schema, perhaps by ignoring the information that might indicate otherwise.

A *complete specification* of a case is the listing of all the details of a case using an interpretation of the case in terms of a given schema. For example, a complete specification of a case of friendship relations using a balanced schema would list all the liking and disliking relationships among the set of people. Notice that if the case were interpreted as fitting a balanced schema, the complete specification could be determined simply by listing to which of the two groups each person belonged.

A specification of a case is never completely certain for a person, so each specification carries a certain degree of confidence; thus a specification of a case represents a belief which is held with more or less confidence by the person.[5] The *accessibility* of a schema for a particular type of case is the order in which that schema is checked for its fit to the information. A highly accessible schema is checked before a fit with a less accessible schema is attempted. The critical assumption of this model is that the person tries to find a schema which provides a satisfactory fit to the case at hand. How this is done is described in the next section.

C. *The Dynamics of the Model*

Figure 1 is a flow-chart of the model. The process starts when a message is received about a case (box 1). The first question is then whether there is already an interpretation of this case (box 2).[6] If there is, then the new information is checked to see if it fits sufficiently well any of the old specifications based on that interpretation (box 3). If it does fit the old interpretation, there is no problem, and the new information gets interpreted with the help of the old schema (box 11). If the new information does not fit the old interpretation, then blame is affixed by comparing the credibility of the source of the new information with the confidence of the old interpretation (box 4). If the new message is blamed, the credibility of the new source is downgraded for future reference, and the process ends with the old interpretation and specification maintained unchanged (box 5). If the old interpretation is blamed, the source of the old information has *its* credibility downgraded, and the old interpretation is cancelled (box 6). Whatever can be remembered of the old information is then taken at face value without any schema to interpret it and is combined with the new information in the new case to provide a partial specification of the case at hand (box 8). This combining process (box 8) would also occur if there had been no previous interpretation of the case but there had been some old uninterpreted information on the case (a "Yes" after 7).

From either path (whether from box 6 or 7), the person gets to the step of seeking a schema which will provide a satisfactory fit to the available information at hand (box 9). This step uses a "satisficing subroutine" which will be described separately because it is so important to the model. If the search process fails to find a schema which provides a sufficiently good fit, the source is downgraded (box 10), and the process ends without an interpretation of the case: i.e., the person is baffled.

If the search for a satisfactory schema is successful, this schema is then used to specify further the case at hand (box 11). The specification stage (box 11) is also reached if an interpretation of the case at hand already existed in terms of some schema and if the new information fit the old specification sufficiently well (i.e., "Yes" after box 2 and "Yes" after box 3). The interpretation is made by first modifying the new information if necessary so that it will coincide exactly with the selected schema. For example, if the selected schema is balance, then all the inconsistencies (such as "A likes B and C, but B doesn't like C") are resolved to correspond to the schema of balance. This gives a modified partial specification of the case. Next this perfectly fitting partial specification is extended with the further use of the selected schema. For example, if the partial specification includes the information that Joe likes Sam, and Sam likes Bill, and the selected

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[5] For simplicity, the present model makes the unrealistic assumption that each of the separate relationships within a specification is believed with the same degree of confidence.

[6] The present model assumes that new information can be assigned to a case and that each case can be assigned to a known case type. Quite possibly these assignments involve a process similar to the interpretation process described within the model.

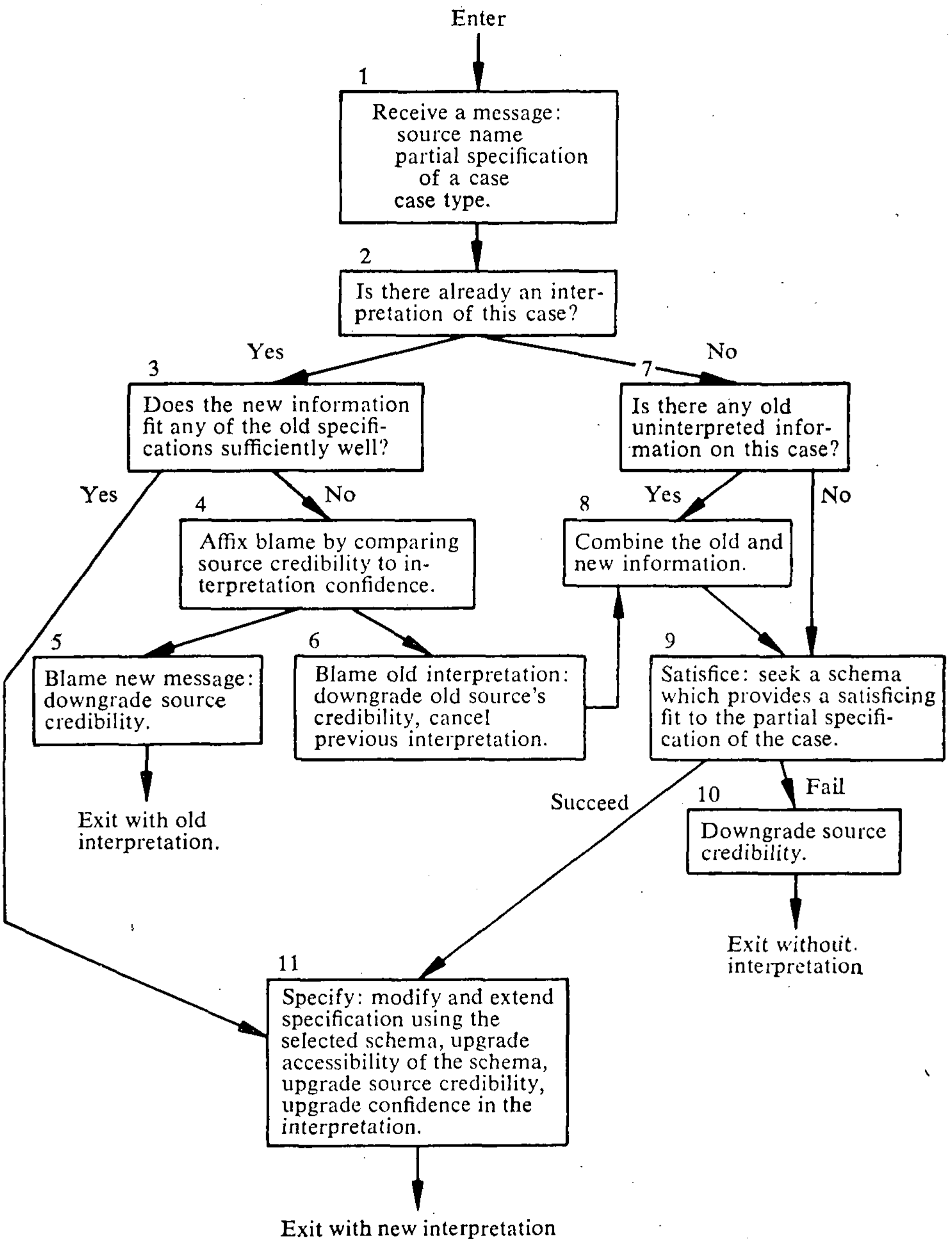
Enter
1
Receive a message: source name partial specification of a case case type.
2
Is there already an interpretation of this case?
Yes
No
3
Does the new information fit any of the old specifications sufficiently well?
7
Is there any old uninterpreted information on this case?
Yes
4
No
Affix blame by comparing source credibility to interpretation confidence.
8
Yes
No
Combine the old and new information.
5
Blame new message: downgrade source credibility.
6
Blame old interpretation: downgrade old source's credibility, cancel previous interpretation.
9
Satisfice: seek a schema which provides a satisficing fit to the partial specification of the case.
Exit with old interpretation.
Succeed
10
Fail
Downgrade source credibility.
Exit without interpretation
11
Specify: modify and extend specification using the selected schema, upgrade accessibility of the schema, upgrade source credibility, upgrade confidence in the interpretation.
Exit with new interpretation

Figure 1. Process Model for Schema Theory.

schema is one of balance, then several inferences can be drawn. One is that Sam likes Joe (since relationships in a balanced schema are symmetric) and another is that Joe likes Bill (since positive relationshps in a balanced schema are transitive). These additional inferences allow the person to extend his specification of the current case.

Together with this modification and extension of the partial specification of the case there are some bookkeeping operations which allow the person to benefit in the future from his experience of finding a satisfactory fit (box 11). He upgrades the accessibility of the selected schema, he upgrades the credibility of the source of the message, and he upgrades the confidence in the current interpretation of the case. If there was already an interpretation of this case and the new information fits this interpretation, then the confidence in that interpretation will be increased. On the other hand, if there was no previous interpretation of the case, then the certainty of the present interpretation depends directly on the credibility of the source of the current message and the goodness of the fit of the new information to the selected schema. Once the interpretation is made and the bookkeeping is performed, the process ends with the new interpretation and specification as the primary output.

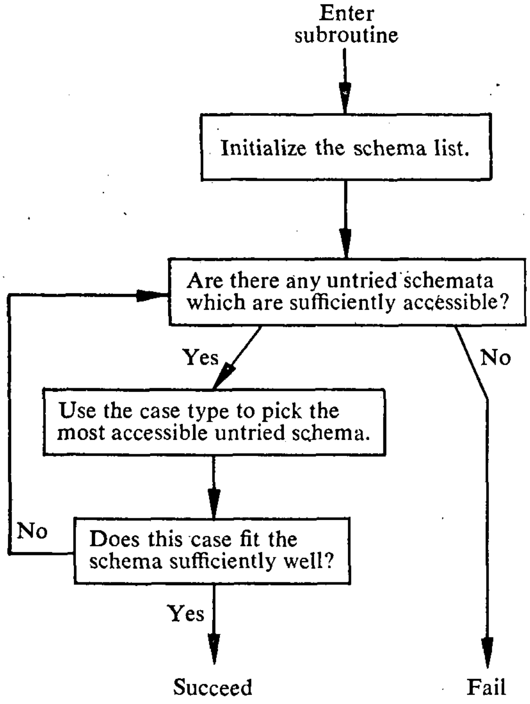
Enter subroutine
Initialize the schema list.
Are there any untried schemata which are sufficiently accessible?
Yes
No
Use the case type to pick the most accessible untried schema.
No
Does this case fit the schema sufficiently well?
Yes
Succeed
Fail

Figure 2. The Satisficing Subroutine.

The satisficing subroutine (box 9) is presented in greater detail in Figure 2. This subroutine describes how a person seeks a schema which will provide a satisfactory fit to the available information on the case at hand. A person does not try out every conceivable schema in an attempt to find the best fit of all. Instead he uses his knowledge of the type of case it is to pick out the most accessible schema for that type of case. Then if the fit with this schema is not sufficiently good, he tries the next most accessible schema, and so on. He continues this process until either he finds a schema which provides a satisfactory fit or he exhausts the schemata which are sufficiently accessible for this type of case. Thus the process of trying to fit the information about a case to a schema is a process of "satisficing" (to use the apt term of Simon)[7] rather than maximizing.

This, then, is the model of how a person makes sense out of a complex world. Once he has interpreted a case in terms of a schema he achieves four things:

(1) He modifies some of the information in the light of his old interpretation (if there was one) and the rest of his information. This allows him to change his beliefs about those parts of the information which may have been wrong for one reason or another. Whether these changes are corrections or distortions depends upon the accuracy of the new interpretation and specification.

(2) He usually achieves a more complete specification of the case than he was given by the information in the original message or messages. This allows him to make predictions about previously unobserved aspects of the case. These predictions can be helpful for decision making since the partial specification of the case (even when modified) might not have constituted an adequate guide for behavior. The degree to which the extended specification can serve as a guide for action depends upon whether the selected schema contains parameters that can help a person identify and choose among future courses of action. For example, a balanced interpretation of a set of friendship relationships which includes oneself is a powerful guide to action, since it allows one to orient oneself postiviely or negatively toward each of the other people without any ambiguity.

(3) He reduces his memory requirements by interpreting the separate bits of information about the current case in terms of the parameters of a schema. For example, it takes n^2 bits of information to remember an arbitrary set of binary friendship relationships between n people, but it takes only $n-1$ bits of information to remember a set of friendship relationships which fits a balanced schema of friends and enemies. The amount of the reduction in the memory requirement depends upon the "size" of the schema, a characteristic which is further explored in VI, A below.

(4) He uses the results of the current case to provide feedback which will affect the processing of information in the future. In particular, he makes adjustments for the future interpretations of this or other cases by updating the accessibility of the schema, the credibility of the source, and the confidence in the specification of the current case. Whether these modifi-

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[7] Herbert Simon, *Models of Man* (New York: Wiley, 1957).

cations will be a help or a hindrance in the future depends upon whether he has drawn the right conclusions from the current message.

Now that the model has been presented, the relevant evidence for this schema theory will be discussed in the next two sections. The first of these sections (Part II) offers the evidence which deals with specific features of the process, as indicated by references to specific boxes in the flow-chart of Figure 1. Part III offers evidence on the overall system performance of the model. These two parts of the paper and the two parts that follow them are written in outline form in order to better display the structure of the presentation.

II. Evidence Relating to Specific Parts of the Model

A. *Use of Prior Interpretations (Figure 1, Boxes 2 and 3)*

1. *Claim:* A prior interpretation of a case (if one exists) is highly accessible.

Evidence: There is a primacy effect in impression formation, which means that a first impression carries more weight than later impressions.[8]

B. *The Blaming Process (Boxes 4, 5, and 6)*

1. *Claim:* When new information gives a bad fit to the old interpretation, one possibility is that the old interpretation can be maintained by downgrading the credibility of the source of the new information.[9]

Evidence: Information which is discrepant with previous beliefs tends to be perceived as less informed and less fair.[10]

2. *Claim:* The greater the source credibility, the more likely the old interpretation is to be blamed for any discrepancy.

Evidence: When the same message is attributed to sources of differing credibility, differing amounts of attitude change are produced with the more credible sources producing the greater attitude change.[11,12]

3. *Claim:* The greater the source credibility, the less likely the source is to be blamed for any discrepancy.

Evidence: A given message is judged more fair and more fully documented when it comes from a high credibility source rather than a low credibility source.[13]

4. *Claim:* Sources whose messages are likely to have been interpreted with an accessible schema tend to have high credibility.

Evidence: Attribution of knowledge, intelligence, education and social status give credibility to an unknown source.[14]

But it is not clear whether the old interpretation and the new source can both be blamed at once.[15]

5. *Claim:* When an old interpretation is blamed, the schema used in the old interpretation becomes somewhat less accessible for future cases of the same type and perhaps for other types of cases as well.

Evidence: Children can learn with experience to switch the order in which they test hypotheses.[16]

But even adults have greater difficulty developing nonlinear hypotheses to explain evidence when linear hypotheses repeatedly fail.[17]

C. *Combining Information (Boxes 7 and 8)*

1. *Claim:* Old and new information about

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[8] S. E. Asch, "Forthcoming Impressions of Personality," *Journal of Abnormal and Social Psychology,* 41 (July, 1946), 258–290; and N. H. Anderson, "Primacy Effects in Personality Impression Formation Using a Generalized Order Effect Paradigm," *Journal of Personality and Social Psychology,* 2 (July, 1965), 1–9.

[9] Note that the present model does not treat the effects of limited or inaccurate memory.

[10] J. E. Dietrich, "The Relative Effectiveness of Two Modes of Radio Delivery in Influencing Attitudes," *Speech Monographs,* 13 (1946), 58–65; C. I. Hovland, O. J. Harvey, and M. Sherif, "Assimilations and Contrast Effects in Communication and Attitude Change," *Journal of Abnormal and Social Psychology,* 55 (September, 1957), 242–252; and Anne S. McKillop, *The Relationship Between the Reader's Attitudes and Some Types of Reading Response* (New York: Columbia University Press, 1952).

[11] L. R. Anderson, "Discrediting Sources as a Means of Belief Defense of Cultural Truisms," *American Psychologist,* 21 (July, 1966), 708.

[12] A more complex model would take account of other source factors as well as credibility. The new information could then be regarded as having a confidence parameter which was determined by factors including the attractiveness and the power of the source as well as the source's credibility. For a review of source factors in attitude change see William J. McGuire, "The Nature of Attitudes and Attitude Change," in *Handbook of Social Psychology,* ed. Lindzey and Aronson, III, 136–314.

[13] C. I. Hovland and W. Mandell, "An Experimental Comparison of Conclusion-drawing by the Communicator and by the Audience," *Journal of Abnormal and Social Psychology,* 47 (July, 1952), 581–588; and C. I. Hovland and W. Weiss, "The Influence of Source Credibility on Communication Effectiveness," *Public Opinion Quarterly,* 15 (Winter, 1951), 635–650.

[14] Carl I. Hovland, Irving L. Janis, and Harold H. Kelley, *Communication and Persuasion* (New Haven: Yale University Press, 1953).

[15] C. E. Osgood and P. H. Tannenbaum, "The Principle of Congruity in the Prediction of Attitude Change," *Psychological Review,* 62 (January, 1955), 42–55.

[16] Neal E. Miller and John Dollard, *Social Learning and Imitation* (New Haven: Yale University Press, 1941).

[17] Mats Bjorkman, "Predictive Behavior: Some Aspects Based on Ecological Orientation," *Scandinavian Journal of Psychology,* 7 (1966), 43–57.

the same case are combined using a weighted average.

Evidence: The impression of a sequence of trait adjectives is the weighted average of the impressions of the separate adjectives and of an assumed nonzero initial impression.[18] The existence of the initial impression is supported by the finding that even without specific information, people have non-neutral expectations, e.g., that two people will like rather than dislike each other.[19]

D. *Selecting a Schema (Boxes 9 and 10)*

1. *Claim:* The selection of a schema with which to interpret information about a case is a process of satisficing rather than maximizing the fit.

Evidence:

a. Some pictures are multistable so that the same person can give several different but temporarily stable interpretations of the same ambiguous picture.[20]

b. The Socratic effect seems to exist in which a person who is asked to state his opinions on related issues changes his opinions to achieve logical consistency.[21,22]

c. Nonrandom distortions are introduced when a person is asked to reproduce a message.[23] For example, incongruous or unusual details are either forgotten *or* exaggerated (presumably because either no schema was found to fit them *or* a relatively inaccessible schema was selected).

Note: There is as yet little evidence to indicate just what type of suboptimal process is used to select a schema from among those which are more or less accessible and which provide a better or worse fit to the initial partial specification of the current case. The use of a satisficing process in a model of schema selection seems suitable but must be regarded as conjectural. For the use of satisficing processes in the choice of alternative courses of action see Simon,[24] March and Simon,[25] and Cyert and March.[26]

2. *Claim:* The accessibility of a schema is independent of the input and output modes.

Evidence: The accessibility of a schema is relatively insensitive to the manner in which the message is presented[27] or the manner in which the response is elicited.[28]

3. *Claim:* The accessibility of a schema depends in part on the case type of the current case.

Evidence: Two identical patterns of relationships are processed differently if different labels are given to them. For example, a pattern of interpersonal relationships which is completely ordered (asymmetric, transitive, and complete) is much easier to learn if the relationships are labeled "influences" rather than "likes."[29] See also III, A, 1.

E. *Modifying the Initial Partial Specification (Box 11)*

1. *Claim:* The initial partial specification is modified in a manner which improves the fit between the partial specification and the selected schema.

Evidence:

a. An assimilation/contrast effect exists in which a subject tends to underestimate the distance between a stimulus and a standard of reference when the distance is small, and overestimate it when the distance is large.[30] The effect has been applied to attitude change,[31] personality functioning,[32] and social perception.[33]

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[18] Anderson, "Primacy Effects"; Zajonc, "Cognitive Theories"; and Rosenberg, "Mathematical Models."

[19] Clinton B. DeSoto and James L. Kuethe, "Subjective Probabilities of Interpersonal Relationships," *Journal of Abnormal and Social Psychology,* 59 (September, 1959), 290–294.

[20] Fred Attneave, "Multistability in Perception," *Scientific American* (December, 1971), pp. 63–71.

[21] This effect was found by William J. McGuire, "Cognitive Consistency and Attitude Change," *Journal of Abnormal and Social Psychology,* 60 (May, 1960), 345–353.

[22] Only weak indications of such an effect were found by R. C. Dillehay, C. A. Insko, and M. B. Smith, "Logical Consistency and Attitude Change," *Journal of Personality and Social Psychology,* 3 (June, 1966), 646–654.

[23] Frederic Bartlett, *A Study in Experimental and Social Psychology* (Cambridge: Cambridge University Press, 1969).

[24] Simon, *Models of Man.*

[25] James G. March and Herbert A. Simon, *Organizations* (New York: Wiley, 1958).

[26] Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963).

[27] James Kuethe, "Pervasive Influence of Social Schemata," *Journal of Abnormal and Social Psychology,* 68 (May, 1964), 248–254.

[28] James Kuethe and Herbert Weingartner, "Male-Female Schemata of Homosexual and Non-Homosexual Penitentiary Inmates," *Journal of Personality,* 32 (March, 1964), 23–31.

[29] Clinton B. DeSoto, "Learning a Social Structure," *Journal of Abnormal and Social Psychology,* 60 (May, 1960), 417–421.

[30] Muzafer Sherif and Carl I. Hovland, *Social Judgment: Assimilation and Contrast Effects in Communication and Attitude Change* (New Haven: Yale University Press, 1961); Carolyn W. Sherif, Muzafer Sherif, and R. E. Nebergall, *Attitude and Attitude Change* (Philadelphia: Saunders, 1965); and Carolyn W. Sherif and Muzafer, Sherif, eds., *Attitude, Ego-Involvement and Change* (New York: Wiley, 1967).

[31] Sherif and Hovland, *Social Judgment.*

b. A stereotyping effect exists in which a subject underestimates the relevant differences within certain groups and overestimates the differences between contrasting groups. Stereotyping occurs not only in the perception of social groups, but even of abstract stimuli such as lengths of variously classified lines[34] and stimuli classified by their positions in an array.[35]

c. Regularization of speech occurs in which children treat strong verbs as it they were weak verbs (e.g., by saying "comed" instead of "came" for the past tense of "come").[36]

d. A common error in recalling an experience is to recall the part that fits a schema and forget the part that does not fit.[37] In general, material is recalled in a manner which indicates that it has been selected, organized and interpreted.[38]

e. Stimuli which have separate names can be more accurately remembered than stimuli which do not (e.g., highly codable colors can be accurately selected from a collection of colors). This holds for the same colors between different languages[39] and for different colors within one language.[40] It may be due to covert verbalization.[41]

f. The extent to which actual friendship groups fit the schema of balance is overperceived.[42]

F. *Expansion of the Partial Specification (Box 11)*

1. *Claim:* The modified partial specification is extended in a manner which does not worsen the fit between the specification and the schema.

Evidence:

a. Guesses based on limited information provide the best possible fits to the appropriate schema (e.g., if "*A* likes *B*" then "*B* likes *A*" is guessed),[43] and in general missing information about friendship patterns is guessed to fit a balanced schema.[44]

b. Guesses in signal detection experiments are consistent with a model of a person as a receiver who is uncertain about the signal rather than as a receiver who has a variable response criterion or who has noise inherent in his sensory system.[45]

c. Errors in long-term recall of liking relationships contained in stories are more likely to be made in recalling imbalanced as balanced patterns than the other way around.[46]

d. The previously noted effect of overperception of balance in actual friendship groups[47] may be due to the extension of the partial specification beyond the information available.

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[32] Leonard Berkowitz, "Judgmental Processes in Personality Functioning," *Psychological Review*, 67 (March, 1960), 130–142.

[33] Leonard Berkowitz and R. E. Goranson, "Motivational and Judgmental Determinants of Social Perception," *Journal of Abnormal and Social Psychology*, 69 (September, 1964), 296–302.

[34] Henri Tajfel and A. L. Wilkes, "Classification and Quantitative Judgement," *British Journal of Psychology*, 54 (May, 1963), 101–114.

[35] D. T. Campbell, "Enhancement of Contrast as Composite Habit," *Journal of Abnormal and Social Psychology*, 53 (November, 1956), 350–355.

[36] Susan Ervin, "Imitation and Structural Change in Children's Language," in *New Directions in the Study of Language*, ed. Eric H. Lenneberg (Cambridge: M.I.T. Press, 1964), pp. 163–190.

[37] R. S. Woodworth, *Experimental Psychology* (New York: Holt, 1938); George A. Miller, "Some Psychological Studies of Grammar," *American Psychologist*, 17 (November, 1962), 748–762.

[38] Bernard R. Berelson and Gary A. Steiner, *Human Behavior* (Burlingame: Harcourt, Brace and World, 1964).

[39] Eric H. Lenneberg and John M. Roberts, *The Language of Experience* (Baltimore: Waverly Press, 1956). Also appeared as *International Journal of American Linguistics*, 22, No. 2; and in Sol Saporta, ed., *Psycholinguistics* (New York: Holt, Rinehart and Winston, 1961).

[40] Roger Brown and Eric H. Lenneberg, "A Study in Language and Cognition," *Journal of Abnormal and Social Psychology*, 49 (July, 1954), 454–462.

[41] Murray Glanzer and William H. Clark, "Accuracy of Perceptual Recall: An Analysis of Organization," *Journal of Verbal Learning and Verbal Behavior*, 1 (January, 1963), 289–299.

[42] Nathan Kogan and Renato Tagiuri, "Interpersonal Preference and Cognitive Organization," *Journal of Abnormal and Social Psychology*, 56 (January, 1958), 113–116.

[43] Clinton E. DeSoto and James L. Kuethe, "Perception of Mathematical Properties of Interpersonal Relations," *Perceptual and Motor Skills*, 8 (December, 1958), 279–286; and DeSoto and Kuethe, "Subjective Probabilities." For a quantitative extension, see A. Rodney Wellens and Donald L. Thistlethwaite, "Comparison of Three Theories of Cognitive Balance," *Journal of Personality and Social Psychology*, 20 (October, 1971), 82–92.

[44] J. Morrissette, "An Experimental Study of the Theory of Structural Balance," *Human Relations*, 11 (August, 1958), 239–254; and Elizabeth G. Schrader and D. W. Lewit, "Structural Factors in Cognitive Balancing Behavior," *Human Relations*, 15 (August, 1962), 265–276.

[45] J. A. Swets, "Is There a Sensory Threshold?" *Science*, 134 (July 21, 1961), 168–177; and D. M. Green, "Psychoacoustics and Detection Theory," *Journal of the Acoustical Society of America*, 29 (1960), 1180–1203. For a review see Clyde H. Coombs, Robyn M. Dawes, and Amos Tversky, *Mathematical Psychology* (Englewood Cliffs, N.J.: Prentice-Hall, 1970).

[46] Harold B. Gerard and Linda Fleischer, "Recall and Pleasantness of Balanced and Unbalanced Structures," *Journal of Personality and Social Psychology*, 7 (November, 1967), 332–337.

[47] Kogan and Tagiuri, "Interpersonal Preference."

III. Predictions of System Performance

A. *Ease of Learning*

1. *Prediction:* Where the initial information provides a good fit to an accessible schema, learning to reproduce the information in a message accurately will be easy.

Evidence:

a. The greater the imbalance in a liking structure, the more errors in learning a semicycle in it.[48]

b. As previously noted, a hypothetical social structure is easiest to learn when it possesses the structural properties expected of it.[49]

c. As previously noted, errors tend to improve the fit between the recalled material and the selected schema (see Evidence for Claim II, E, 1 above).

2. *Prediction:* When an excellent fit is required, the less accessible the appropriate schema is, the more difficult the problem will be to solve.

Evidence:

a. The unfamiliar use of the familiar is a difficult solution to achieve.[50]

b. When a prior success is inapplicable, the problem is difficult to solve.[51]

3. *Prediction:* When the initial information provides a good fit to an accessible schema there will be little manifestation of surprise.

Evidence:

a. Messages which provide a good fit to a balanced liking schema are judged to be persuasive, accurate, and pleasant.[52]

b. Messages which are incongruous cause physiological arousal (e.g., as measured by galvanic skin response).[53]

B. *Attitude Change*

1. *Prediction:* Old beliefs are changed in accordance with the model, i.e., when information in a new message does not fit the old interpretation, a comparison of the source credibility of the new message with the confidence in the old interpretation can result in blaming the old interpretation.

Evidence:

a. Repeating the same argument is no help in attitude change.[54]

b. Internalization occurs in attitude change, which means that if the source of a persuasive message is forgotten, the changed attitude is still retained. Even if the source is known to switch his position, the attitude change remains.[55]

c. Over a wide range, the larger the advocated change, the greater the change in attitude, provided the source is regarded as credible.[56]

C. *Effect of Experience*

1. *Prediction:* A highly accessible schema will only be downgraded if reliable information is acquired which does not fit the schema (since no other schema will get priority in the search process).

Evidence: A dog taught to jump at regular intervals to avoid a shock continues to jump long after the shock mechanism is turned off. In order to unlearn the lesson, he has to be physically prevented from jumping.[57]

2. *Prediction:* When the interpretation of a sequence of cases repeatedly uses the same schema, the schema becomes more accessible.

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[48] R. B. Zajonc and E. Burnstein, "Structural Balance, Reciprocity, and Positivity as Sources of Cognitive Bias," *Journal of Personality,* 33 (December, 1965), 570–583. For related results on the learning of liking structures, see R. B. Zajonc and E. Burnstein, "The Learning of Balanced and Unbalanced Social Structures," *Journal of Personality,* 33 (June, 1965), 153–163.

[49] DeSoto, "Learning a Social Structure." For more recent verification that learning a set of relationships is easier when they fit the expected structure, see Clinton B. DeSoto, Nancy M. Henley, and Marvin London, "Balance and the Grouping Schema," *Journal of Personality and Social Psychology,* 8 (July, 1968), 1–7; Nancy M. Henley, Robert B. Horsfall and Clinton B. DeSoto, "Goodness of Figure and Social Structure," *Psychological Review,* 76 (1969), 194–204; and Robert B. Horsfall and Nancy M. Henley, "Mixed Social Structures: Strain and Probability Ratings," *Psychonomic Science,* 15 (May, 1969), 186–187; Donald L. Mosher, "The Learning of Congruent and Noncongruent Social Structures," *Journal of Social Psychology,* 73 (December, 1967), 285–290.

[50] Berelson and Steiner, *Human Behavior,* p. 204.

[51] Berelson and Steiner, *Human Behavior,* p. 204.

[52] Milton J. Rosenberg and Robert P. Abelson, "An Analysis of Cognitive Balancing," in *Attitude Organization and Change,* Milton J. Rosenberg, Carl I. Hovland, William J. McGuire, Robert P. Abelson and Jack W. Brehm (New Haven: Yale University Press, 1960), pp. 112–163.

[53] D. E. Berlyne, *Conflict, Arousal, and Curiosity* (New York: McGraw-Hill, 1960), and Evgenii N. Sokolov, *Perception and the Conditioned Reflex,* trans. Stephen W. Waydenfeld (Oxford and New York: Pergamon, 1963).

[54] McGuire, "The Nature of Attitudes and Attitude Change," p. 169.

[55] Sherif, "A Study of Some Social Factors in Perception."

[56] See McGuire, "The Nature of Attitudes and Attitude Change," p. 223, for a review.

[57] R. L. Solomon, L. J. Kamin, and L. C. Wynne, "Traumatic Avoidance Learning: The Outcomes of Several Extinction Procedures with Dogs," *Journal of Abnormal and Social Psychology,* 48 (April, 1953), 291–302.

Therefore, it will eventually be selected even when other less accessible schemata might provide better fits.

Evidence: As previously noted, children can learn to switch the order in which they test hypotheses.[58]

D. *Resolution of Cognitive Inconsistencies*

1. *Prediction:* When some information (whether old or new) fits none of the sufficiently accessible schemata precisely, employable tactics include the following: blame the source, use the other old and new information about the case to select a schema which gives a sufficiently good fit to all the available information, or exit without an interpretation.

Evidence: These modes are among the many which have been suggested (with varying degrees of empirical support) as ways of resolving cognitive inconsistencies.[59]

E. *Measuring Accessibility of a Schema*

The accessibility of a schema for a particular type of case is not directly observable. It can be measured indirectly using any one of the four methods described above: ease of learning, attitude change, effects of experience, and resolution of cognitive inconsistencies. When studying a new person or a new type of case, one method can be used to validate another method's results.

IV. Comparison with Other Theoretical Approaches

A. *Computer Simulations*

To my knowledge, no computer simulation of human perception or cognition treats the differential accessibility of alternative schemata, which is the heart of the present model.[60]

B. *Gestalt School of Psychology*

The Gestalt School[61] provides some of the fundamental ideas which the present information processing model tries to develop. The common criticism that the Gestalt approach has lacked precision is well founded.

C. *Information Theory*

Information theory[62] provides some useful mathematical tools, including a way to measure the size of a schema (for a few details see Part VI, A). The application of information theory to formal problems in statistical inference is by now quite well developed.[63]

D. *Balance Theory*

The predictions of balance theory[64] are consistent with the schema theory for those and only those situations in which the balanced schema is the one selected (see VI, B).[65]

1. *Claim:* Balance is a highly accessible schema in certain contexts, such as friendship relationships.[66]

Evidence (all previously cited):

a. Balanced messages are more acceptable than unbalanced messages.[67]

b. Incomplete specifications are extended with the use of a balanced schema.[68]

c. Balance is overestimated.[69]

d. A balanced message is easier to learn and to retain over time.[70]

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[58] Miller and Dollard, *Social Learning and Imitation.*

[59] William J. McGuire, "The Current Status of Cognitive Consistency Theories," in *Cognitive Consistency: Motivational Antecedents and Behavioral Consequents,* ed. Shel Feldman (New York: Academic Press, 1966); Robert P. Abelson, "Psychological Implication," in Abelson et al., *Theories of Cognitive Consistency,* pp. 112–139; Abelson et al., *Theories of Cognitive Consistency,* chapters 64–71; and Percy H. Tannenbaum, Jacqueline R. Macaulay, and Eleanor L. Norris, "Principle of Congruity and Reduction of Persuasion," *Journal of Personality and Social Psychology,* 3 (February, 1966), 233–238.

[60] But for some interesting simulations of mental processes see Silvan Tomkins and Samuel Messick (eds.), *Computer Simulation of Personality* (New York: Wiley, 1963), Robert P. Abelson and J. Douglas Carroll, "Computer Simulation of Individual Belief Systems," *American Behavioral Scientist,* 8 (May, 1965), 24–30; John C. Loehlin, *Computer Models of Personality* (New York: Random House, 1968); U. Moser, I. S. von Zeppelen and W. Schneider, "Computer Simulation of a Model of Neurotic Defence Processes," *Behavioral Science,* 15 (March 1970), 194–202; and Allen Newell and Herbert Simon, "Simulation of Human Thought," in *Computer Simulation of Human Behavior,* ed. John M. Dutton and William Starbuck (New York: Wiley, 1971).

[61] Max Wertheimer, *Productive Thinking* (New York: Harper, 1945).

[62] Wendell R. Garner, *Uncertainty and Structure as Psychological Concepts* (New York: Wiley, 1962).

[63] Satosi Watanabe, *Knowing and Guessing: A Quantitative Study of Inference and Information* (New York: Wiley, 1969).

[64] Robert P. Abelson and Milton J. Rosenberg, "Symbolic Psychologic: A Model of Attitudinal Cognition," *Behavioral Science,* 3 (January, 1958), 1–13.

[65] For a basic introduction to balance theory, see Roger Brown, *Social Psychology* (Glencoe, Ill.: The Free Press, 1956); for a review of most of the experimental evidence see Zajonc, "Cognitive Theories"; for a fund of source material, see Abelson et al., *Theories of Cognitive Consistency.*

[66] In fact, much of the experimental work on schemata cited in this paper has been inspired more or less directly by the study of interpersonal relationships contained in Fritz Heider, *The Psychology of Inter-personal Relations* (New York: Wiley, 1958).

[67] Rosenberg and Abelson, "An Analysis of Cognitive Balancing."

[68] Morrissette, "An Experimental Study."

[69] Kogan and Tagiuri, "Interpersonal Preference."

[70] Clinton B. De Soto, Nancy M. Henley and Marvin London, "Balance and the Grouping Schema," *Journal of Personality and Social Psychology,* 8 (January, 1968),

e. Balanced messages are viewed as less likely to change than unbalanced messages.[71]

f. Balance is an accessible schema in perceived friendship relationships among a set of countries.[72]

g. Balanced situations are imagined more vividly, rapidly and elicit higher affective ratings than unbalanced situations.[73]

2. *Claim:* The limitations of balance theory can be understood in terms of the availability of schemata more accessible than the balance schema which can provide adequate fits to the information on a given case.

Evidence:

a. The schema of positivity (consisting of nothing but positive relationships) can explain some of balance theory's failures, such as

(1) positive links are easier to learn than negative links,[74]

(2) positive semicycles are easier to learn than negative semicycles,[75] and

(3) there is not a large negative correlation between the first two links of a triad when the third is negative.[76]

(4) subjects' willingness to change the relationships between themselves and a fictitious person is determined by positivity forces.[77]

b. If a subject likes chicken, and chickens like chicken feed, the subject is not expected to like chicken feed.[78]

c. If subjects are told that two boys like a girl, the subjects do not expect them to like each other as much as if they both like a third boy.[79]

d. If told that two people like *Newsweek* magazine, subjects do not mistakenly learn that they like each other, but this mistake is made when two people are said to like racial integration.[80]

3. *Claim:* A balanced schema is selected when appropriate, even outside of the context suggested by balance theory.

Evidence: The concepts used by decision makers in describing their images of external reality are balanced.[81] A balanced schema is applied in tasks of recall and attribution of responsibility.[82]

4. The schema theory is consistent with all of the following explanations for the tendency toward cognitive consistency: a motivational drive toward consistency, limited cognitive capacity resulting in the need for simplification of incoming information, and inadequacies in the incoming information which must therefore be processed in a manner that allows predictions to be made and later tested.

V. Development of Schema Theory

A. *General Principles of Schema Accessibility*

1. *Claim:* People seek logical consistency.

Evidence:

a. Arguments against the premise of a syllogism will affect the belief in the conclusion a week later.[83]

b. Attitude change by posthypnotic sug-

1–7. Nicolas B. Cottrell, Larry H. Ingraham and Franklin W. Monfort, "The Retention of Balanced and Unbalanced Cognitive Structures," *Journal of Personality*, 39 (March, 1971), 112–131.

[71] Eugene Burnstein, "Sources of Cognitive bias in the Representation of Simple Social Structures; Balance, Minimal Change, Positivity, Reciprocity, and the Respondent's Own Attitude," *Journal of Personality and Social Psychology*, 7 (September, 1967), 36–48. Harold Miller and Dennis Geller, "Structural Balance in Dyads," "*Journal of Personality and Social Psychology*, 21 (February, 1972), 135–138.

[72] Nicholas B. Johnson, "Some Models of Balance Applied to Children's Perceptions of International Relations," *European Journal of Social Psychology*, 2 (1972), 55–64.

[73] Edward B. Blanchard, Marilyn Vickers and Katina C. Price, "Balance Effects in Image Formation," *The Journal of Social Psychology*, 87 (June, 1972), 37–44.

[74] Zajonc and Burnstein, "The Learning of Balanced and Unbalanced Social Structures" and "Structural Balance."

[75] Zajonc and Burnstein, "Structural Balance."

[76] William M. Wiest, "A Quantitative Extension of Heider's Theory of Cognitive Balance Applied to Interpersonal Perception and Self-Esteem," *Psychological Monographs*, 79, No. 1 (Whole No. 607) (December, 1965).

[77] Aroldo Rodriguez, "Effects of Balance, Positivity and Agreement in Triadic Social Relations," *Journal of Personality and Social Psychology*, 5 (April, 1967), 472–476.

[78] R. B. Zajonc, "The Concepts of Balance, Congruity, and Dissonance," *Public Opinion Quarterly*, 24 (Summer, 1960), 280–296.

[79] Robert B. Zajonc and Steven J. Sherman, "Structural Balance and the Induction of Relations," *Journal of Personality*, 35 (December, 1967), 635–650.

[80] Zajonc and Burnstein, "The Learning of Balanced and Unbalanced Social Structures." The authors explain this result by saying that one issue was less important than the other, but an alternative explanation is that the balance schema is not accessible for a case type involving readership of a general purpose magazine.

[81] Axelrod, "Psycho-Algebra."

[82] N. T. Feather, "Organization and Discrepancy in Cognitive Structures," *Psychological Review*, 78 (September, 1971), 355–379.

[83] William J. McGuire, "Cognitive Consistency and Attitude Change"; William J. McGuire, "A Syllogistic Analysis of Cognitive Relationships," in *Attitude Organization and Change*, Milton J. Rosenberg et al. (New Haven: Yale University Press, 1960), pp. 65–111; R. C. Dillehay, C. A. Insko, and M. B. Smith, "Logical Consistency and Attitude Change," *Journal of Personality and Social Psychology*, 3 (June, 1966), 646–654.

gestion causes cognitive reorganization for the maintenance of consistency.[84]

c. People with inconsistent associative structures are easier to change by reinforcement.[85]

d. Logical "mistakes" are often due to the use of different premises from the ones intended.[86]

e. As previously mentioned (II, D, 1, b), there may be a Socratic effect in which a person who is asked to state his opinions on related issues changes his opinions to achieve logical consistency.

2. *Claim:* There is a great deal of culturally stored information and knowledge (especially in the language of a society as Whorf emphasizes).[87] Even beyond this culturally stored information, the accessibilities of schemata are determined in part by factors other than direct observation or interpersonally transmitted experience.

Evidence:

a. Linguistic and other types of cognitive development proceed in relatively fixed stages, which suggests that some types of general rules are easier to learn than others.[88]

b. There seem to be some linguistic universals which are nonfunctional principles of grammar common to all languages. For example, grammatical operations apply in a cyclic fashion, first to the most deeply embedded structures, then to the structures that contain them, and so on.[89]

c. There seems to be a working model of three-dimensional space within the human nervous system which provides the capacity to interpret visual images.[90]

3. *Claim:* Schemata which are readily accessible for the interpretation of sets of relationships tend to have simple mathematical properties.

Evidence: There is empirical evidence for the accessibility of schemata with the following mathematical properties:

a. positive (see IV, D, 2, a above).[91]

b. symmetric.[92]

c. transitive.[93]

d. balanced (see IV, D, 1 and 3 above).

e. acyclic (no cycles, i.e., no set of relationships such that *aRb, bRc, cRd, . . . , xRy, yRz,* and *zRa*).[94]

f. linearly ordered (irreflexive, asymmetric, transitive, and complete).[95]

g. singly ordered (several dimensions collapsed into one linear order).[96]

4. *Claim:* A general feature of schemata is that a new case can be extended by using the parametric values of the specification of old relevant cases (which is a kind of generalization process).

Evidence:

a. Subjects systematically go beyond assuming attitudes which are sufficient to explain specific actions they observe, imputing to the actor attributes of trans-specific generality.[97]

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[84] Milton J. Rosenberg and C. W. Gardner, "Case Report: Some Dynamic Aspects of Posthypnotic Compliance," *Journal of Abnormal and Social Psychology,* 57 (November, 1958), 351–366.

[85] W. A. Scott, "Cognitive Consistency, Response Reinforcement, and Attitude Change," *Sociometry,* 22 (September, 1959), 219–229.

[86] Mary Henle, "On Error in Deductive Reasoning," *Psychological Reports,* 7 (August, 1960), 80.

[87] Benjamin L. Whorf, *Language, Thought and Reality,* ed. J. B. Carroll (Cambridge, Mass.: M.I.T. Press, 1956).

[88] For a review of linguistic development in children see Roger Brown, *Social Psychology* (Glencoe, Illinois: The Free Press, 1956). For a review of Piaget's work on cognitive development see John H. Flavell, *The Developmental Psychology of Jean Piaget* (Princeton: Van Nostrand, 1963).

[89] A recent formulation of the claim for linguistic universals which uses this example is Noam Chomsky, *Problems of Knowledge and Freedom* (New York: Pantheon Books, 1971). For a review of psycholinguistic research see Miller and McNeill, "Psycholinguistics."

[90] Attneave, "Multistability in Perception."

[91] Concept formation experiments provide further indirect evidence. See, for example, Jerome S. Bruner, Jacqueline J. Goodnow and George A. Austin, *A Study of Thinking* (New York: Wiley, 1956).

[92] For example, DeSoto and Kuethe, "Perception of Mathematical Properties"; DeSoto and Kuethe, "Subjective Probabilities"; and DeSoto, "Learning a Social Structure."

[93] For example, DeSoto and Kuethe, "Perception of Mathematical Properties"; DeSoto and Kuethe, "Subjective Probabilities;" and DeSoto, "Learning a Social Structure."

[94] Henley, Horsfall and DeSoto, "Goodness of Figure," Axelrod, "Psycho-Algebra."

[95] Henley, Horsfall and DeSoto, "Goodness of Figure"; Clinton DeSoto and Frank Albrecht, "Conceptual Good Figures," in *Theories of Cognitive Consistency,* Abelson et al., pp. 504–511; and Clinton DeSoto and Frank Albrecht, "Cognition and Social Orderings," in *Theories of Cognitive Consistency,* Abelson et al., pp. 531–538; Jean Pierre Poitou and David Van Kreveld, "Is There a Position Bias in Learning of Influence Structures?" *European Journal of Social Psychology,* 2, No. 1 (1972), 75–85. For a related schema called proximity, see Edmund R. Peay, "Extensions of Clusterability to Quantitative Data with an Application to the Cognition of Political Attitudes" (Ph.D. diss., University of Michigan, 1970).

[96] Clinton B. DeSoto and John J. Bosley, "The Cognitive Structure of a Social Structure," *Journal of Abnormal and Social Psychology,* 64 (April, 1962), 303–307; DeSoto and Albrecht. "Conceptual Good Figure," and DeSoto and Albrecht, "Cognition and Social Orderings."

[97] C. Norman Alexander, Jr., and Joyce Epstein,

b. Subjects accept verbal generalizations even when they know of exceptions to them.[98]

B. *Unanswered Questions*

Among the questions which need to be answered in order to construct a more precise theory of schemata are the following:

1. How does one set the threshold for the level of goodness of fit which is regarded as providing a satisfactory interpretation of a case by a given schema? (See V, C, 1 below on the role of personality and VI, B below on the role of the reliability of the initial information.)
2. If an old interpretation of a case is canceled, is the old schema made less accessible?
3. When an old interpretation does not fit new information, is the blame ever divided between the two (see II, B, 4 above)?
4. Can goodness of fit be treated as a metric (distance function) on the abstract space of specifications of cases (see VI, B below), and if so what are the characteristics of this metric?
5. When and how does a person invent an entirely new schema?

C. *Psychological Implications*

Some psychological results which might be dealt with by an expanded version of the present model are the following:

1. Personality variables. Individual differences in cognitive complexity,[99] authoritarianism,[100] intolerance for ambiguity[101] and autism/realism[102] might be due to different thresholds of minimum goodness of fit. For example, authoritarians are likely to resist mildly derogatory information about a respected partner,[103] thereby maintaining an accessible interpretation at the cost of a good fit.
2. Cognitive dissonance. Dissonance effects include the result that when a boring task is voluntarily chosen by a subject, the less the reward, the *more* the task is valued. This type of result might be attributable to the accessibility of a schema incorporating the consistency of self-behavior.[104]
3. Wishful thinking. The correlation of one's expectations with one's desires[105] might reflect an accurate generalization of past experience even when it seems inappropriate in an artificial setting.

VI. Applications in Normative Analysis

An information-processing model can be very helpful for suggesting new questions for further research as well as for integrating a variety of findings from experimental and social psychology. But from the viewpoint of political science, perhaps the greatest benefit from this type of model will come from its ability to provide a framework for the analysis of actual behavior in important arenas of human action. If we are able to identify the characteristics of optimal performance and if we are able to compare actual performance to optimal performance, then we can both improve our understanding of people's actual performance and perhaps offer some helpful advice to those who wish to improve their own performance.

The process of selecting a schema with which to interpret a case offers two opportunities for this type of analysis. The selection process within the model is a satisficing procedure which does not necessarily yield the best possible fit between the selected schema and the information available about the current case—nor should it necessarily, as will be shown. In addition to the information about the current case, the best use of the satisficing procedure requires at least two types of parameters. These are (1) the relative accessibilities of alternative schemata and (2) the magnitude of what constitutes a satisfactory fit. Analyzing how these two parameters should be set will help to identify the characteristics of optimal performance.

A. *Optimal Accessibilities of Schemata*

A particularly illuminating question for normative analysis using schema theory is how to

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"Problems of Dispositional Inference in Person Perception Research," *Sociometry*, 32 (December, 1969), 381–395.

[98] Robert P. Abelson and David E. Kanouse, "Subjective Acceptance of Verbal Generalization," in *Cognitive Consistency*, ed. Shel Feldman (New York: Academic Press, 1966), pp. 171–197.

[99] James Bieri and Edward Blacker, "The Generality of Cognitive Complexity in the Perception of People and Inkblots," *Journal of Abnormal and Social Psychology*, 53 (July, 1956), 112–117.

[100] Brown, *Social Psychology*.

[101] Else Frenkel-Brunswik, "Intolerance of Ambiguity as an Emotional and Perceptual Personality Variable," *Journal of Personality*, 18 (September, 1949), 103–143.

[102] Theodore M. Newcomb, "Stabilities Underlying Changes in Interpersonal Attraction," *Journal of Abnormal and Social Psychology*, 66 (April, 1963), 376–386, esp. p. 385.

[103] Ivan D. Steiner and Homer H. Johnson, "Authoritarianism and 'Tolerance of Trait Inconsistency'," *Journal of Abnormal and Social Psychology*, 67 (October 1963), 388–391.

[104] Leon Festinger, *A Theory of Cognitive Dissonance* (Stanford: Stanford University Press, 1957); Jack W. Brehm and A. R. Cohen, *Explorations in Cognitive Dissonance* (New York: Wiley, 1962); and Zajonc, "Cognitive Theories in Social Psychology."

[105] Hadley Cantril, "The Prediction of Social Events," *Journal of Abnormal and Social Psychology*, 33 (1938), 364–389; D. McGreor, "The Major Determinants of the Prediction of Social Events," *Journal of Abnormal and Social Psychology*, 33 (1938), 179–204; and McGuire, "Cognitive Consistency and Attitude Change."

assign relative accessibilities to a given set of schemata. In other words, given a set of schemata, which one should be tested first against new information of a specific type of case, which second, and so on. The answer involves the tradeoff between two competing values which are both desirable in an accessible schema. These two valued characteristics of a schema are veridicality and smallness of size.[106]

The *veridicality* of a schema is the subjective probability that a case of a specific type is in fact an instance of that schema. Obviously, the person wants to interpret new information as fitting a highly veridical schema since such an interpretation would make it likely (at least in the sense of subjective probability) that his modification and extension of the initial partial specification of the case would be accurate rather than erroneous.

The second value desired in an accessible schema is smallness of size. The *size* of a schema is the number of different complete specifications which are instances of the schema. For example, the size of the balance schema is given by the number of different ways of dividing a group of people into two groups (such as friends and enemies). The smaller the size of a schema, the more can be deduced about a case which is interpreted in terms of that schema.

The veridicality of a schema depends on the type of case (see II, D, 3), but the size of the schema is solely a function of the nature of the schema itself. A useful measure of the smallness of a schema is given by the information theory concept of redundancy.[107] For present purposes it is sufficient to note that interpreting a case in terms of a small schema has the advantage of allowing a greater amount of modification and extension of the original partial specification of the case.

A numerical illustration will help make these ideas clear. Consider a set of binary relationships between five objects (such as friendship relationships between five people). Any complete specification of such a case can be represented as a 5 by 5 matrix with 1 and -1 entries (although for convenience a -1 will be shown as a blank in Figure 3 below). For example, the entry in the second row and fourth column could represent whether or not the second person likes the fourth person.

Using this representation, a schema can be defined as the set of all matrices which satisfy certain specified properties. The number of different complete specifications for a schema can then be counted to give the exact size of that schema. Examples of specifications which fit different schemata are shown in Figure 3. The order of the rows and columns has been chosen to best display the defining characteristics of the different schemata.

The null schema is the one which includes all possibilities. There are 5^2 or 25 entries in the matrix, so there are a total of 2^{25} or about one hundred million possible specifications of the null schema. One of these is shown in Figure 3a.

The balance (or 2-cluster) schema has a size of 16, since there are $2^5/2$ ways of putting 5 people into one of no more than two groups, when the order of the groups does not matter. An example is shown in Figure 3b.

Figure 3c displays one of the specifications of the 3-cluster schema. There are 41 ways of putting 5 people into one of no more than three groups ($41 = (3^5 - 3)/3! + 1$).

The positive schema has only one specifica-

$$\begin{array}{|ccccc|}\hline 1 & & 1 & 1 & \\ 1 & 1 & 1 & & \\ 1 & & 1 & 1 & 1 \\ & 1 & & 1 & \\ 1 & & & 1 & \\ \hline \end{array}$$

a.
Null

$$\begin{array}{|ccccc|}\hline 1 & 1 & 1 & & \\ 1 & 1 & 1 & & \\ 1 & 1 & 1 & & \\ & & & 1 & 1 \\ & & & 1 & 1 \\ \hline \end{array}$$

b.
Balance

$$\begin{array}{|ccccc|}\hline 1 & 1 & & & \\ 1 & 1 & & & \\ & & 1 & 1 & \\ & & 1 & 1 & \\ & & & & 1 \\ \hline \end{array}$$

c.
3-cluster

$$\begin{array}{|ccccc|}\hline 1 & 1 & 1 & 1 & 1 \\ 1 & 1 & 1 & 1 & 1 \\ 1 & 1 & 1 & 1 & 1 \\ 1 & 1 & 1 & 1 & 1 \\ 1 & 1 & 1 & 1 & 1 \\ \hline \end{array}$$

d.
Positive

$$\begin{array}{|ccccc|}\hline 1 & 1 & 1 & 1 & 1 \\ & 1 & 1 & 1 & 1 \\ & & 1 & 1 & 1 \\ & & & 1 & 1 \\ & & & & 1 \\ \hline \end{array}$$

e.
Linear order

Figure 3. Sample Specifications of Schemata of Relationships Between Five People.

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[106] For a formal model with a similar tradeoff, see Shun-ichi Amari, "Theory of Learning Decision Systems," in *Unifying Study of Basic Problems in Engineering and Physical Sciences by Means of Geometry,* ed. Kazoo Kondo (Division 1), (Tokyo: Gakujutsu Bunken Fukyu-kai, 1969), pp. 33–42 (which is RAAG Memoirs, 4, [1969], 567–576).

[107] With the assumption that each instance of a schema has equal subjective probability, size determines redundancy. The assumption is based on the principle that if some instances have a higher subjective probability than others, they should be treated by the model as belonging to different schemata. For applications of redundancy to psychology see Garner, *Uncertainty and Structure;* and Wendell R. Garner and David E. Clement, "Goodness of Pattern and Pattern Uncertainty," *Journal of Verbal Learning and Verbal Behavior,* 2 (December, 1963), 446–452. For a recent critical review see D. W. J. Corcoran, *Pattern Recognition* (Harmondsworth, England: Penguin Books, 1971).

tion (shown in Figure 3d), since there is only one way that everyone can like each other.

The schema of linear order has 5! or 120 specifications since there are that many ways of arranging 5 people in order without ties. Such a schema would be appropriate if the case were about relationships of height rather than friendship. Figure 3e illustrates this schema with the order of the rows and columns chosen to display best the pattern.[108]

The tradeoff between veridicality and smallness among schemata is illustrated in Figure 4 for a given type of case, namely friendship relationships. A highly accessible schema should be both veridical and small. The balance (or 2-cluster) schema is moderately small and has fairly good veridicality, since the person presumably knows that friendship groups do tend to be balanced.[109] The positive schema is very small indeed, but is not very veridical. The 3-cluster is assumed in this example to be more veridical than the postive schema and is certainly more veridical than the balance schema, but it is larger than either of them. The null schema is certain to be totally veridical, but it is no help in the modification and extension of a partial specification of a case, since it is so large as to include every possibility. The schema of linear order is of large size and is assumed to have low veridicality in this example since a linear order is asymmetric (if one individual has the relationship to a second, then the second can not have that relationship to the first), whereas actual liking relationships tend to be symmetric.

The dotted line of Figure 4 represents the frontier of knowledge about this type of case using these schemata. The growth of knowledge about a type of case could be reflected in the discovery of a schema which was above and to the right of this line and which thereby pushed out the frontier of knowledge.

Pushing out the frontier of knowledge may be one of the tasks of the social scientist, but the person who is actually trying to make sense out of a complex world is often more interested in just what he should do *today* when the frontier of knowledge is at a given place. The answer in terms of Figure 4 is that he should assign high accessibilities to schemata which are high and to the right in Figure 4, medium accessibilities to schemata which are in the upper left or lower right, and low accessibilities to schemata in the lower left.[110]

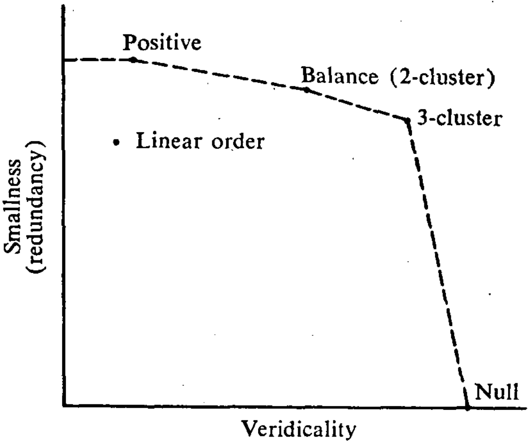

Figure 4. Illustration of the Tradeoff Between Smallness and Veridicality of Different Schemata for Cases of Friendship Relationships.

B. *Optimal Threshold of an Acceptable Fit*

Setting the relative accessibilities of schemata determines the order in which they will be checked for their fit to the current case. Given this order of checking, one may ask what should constitute a satisfactory fit to bring a successful end to the satisficing checking process? In other words, what should be the threshold of how bad a schema's fit can be and still be acceptable as the interpretation of the present case?

To deal with this question a geometric analysis is helpful. To start with, a complete specification of a case can be represented as a single point in an abstract space, to be called a *Q*-space. In this space there is a distance function (or metric) which measures how similar two complete specifications are to each other. Thus if x and y are two points in the *Q*-space, then $d(x,y)$ is a real number indicating the degree to which these two complete specifications are similar or dissimilar to each other.[111]

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[108] For schemata of infinite size (e.g., with continuous rather than discrete parameters), the appropriate measure of size uses degrees of freedom. For example, a linear equation has two degrees of freedom (namely a and b in $y = ax + b$), and a quadratic equation has three degrees of freedom (namely a, b and c in $y = ax^2 + bx + c$).

[109] Kogan and Tagiuri, "Interpersonal Preference and Cognitive Organization."

[110] To carry the analysis further one could do the type of indifference curve analysis used in microeconomics. Negatively sloped indifference curves could be drawn in Figure 4 to represent the tradeoffs between smallness and veridicality. Then the schemata should be given accessibility in order of their intersection with the ordered set of indifference curves. If there are enough alternative schemata to allow for a continuous approximation of the frontier of knowledge, then the schema which should be given the highest accessibility is represented by the point at which one of the indifference curves is tangent to the frontier of knowledge.

[111] A metric function has the following properties:

The distance function can be applied to pairs of sets in Q by taking the minimum distance between members of the two sets. For example, let X be a partial specification of a case (i.e., a subset of Q consisting of all complete specifications which are consistent with the information in the partial specification), and let S be a schema (so that S is also a subset of Q by definition). Then define $d(X,S) = \min d(x,y)$ for x in X and y in S. Less formally, the distance between a partial specification and a schema is how close they come together.[112]

There are four immediate uses to which the distance function can be put. First, goodness of fit can be defined as the distance between an initial partial specification and a schema, namely $d(X,S)$. Second, a satisfactory fit can be defined by the condition that this distance is less than or equal to k for some fixed k. Third, the selected schema is the first schema such that $d(X,S_i)$ is less than or equal to k, where S_1 is the most accessible schema, S_2 is the second most accessible schema and so on. Fourth, the modification and extension of a partial specification is a set of points, X', defined in the following manner. X' is the set of all x' in the selected schema, S', such that $d(X,\{x'\}) = d(X,S')$. In other words, the modified and extended specification of a case is the set of all points in the selected schema which are at minimal distance from the initial partial specification. The idea is that the initial specification is altered as little as possible in order to make it fit the selected schema.

An abstract illustration of these ideas is provided by Figure 5. In this figure there are three schemata labeled in order of their accessibility, S_1, S_2 and S_3. There is also a partial specification of a case, X. Shown near the bottom is a scale of the distance, k, which is the maximum allowable for a satisfactory fit. (For convenience here, the metric in Figure 5 can be thought of as a normal Euclidean distance, but other metrics are certainly possible.)

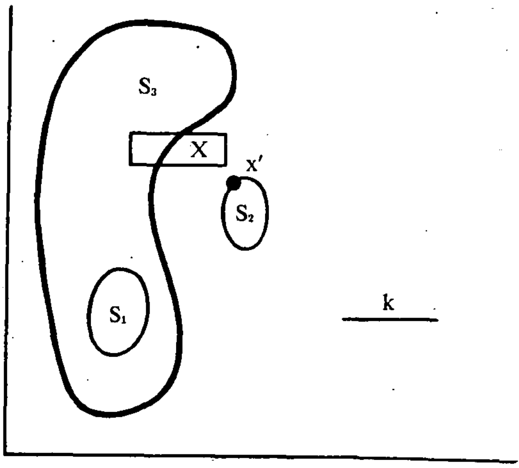

Figure 5. Illustration of the Q Space.

X is the initial partial specification of the case
S_1, S_2 and S_3 are schemata
k is the maximum distance of a satisfactory fit
x' is the final specification of the case using the selected schema, S_2

Some of the key operations of the information-processing model can be illustrated with the help of this geometric example. The satisficing routine proceeds as follows. S_1 is not selected because all of its points are too far from any of the points in X. S_2 *is* selected as the interpretation of the case because some of the points in X are within a distance of k from some of the points in S_2. S_3 is never even checked by the satisficing subroutine and therefore cannot be selected even though S_3 provides the best fit in this example. The specification routine proceeds by picking the points in the selected schema, S_2, which are closest to X. In this example, there is a unique such point, x', and thus the specification process results in a complete specification of the case as well as an interpretation of the case as belonging to the S_2 schema.

The familiar case type of friendship relationships helps demonstrate some of the properties a Q-space can have. For simplicity consider a set of friendship relationships among three people which take on one of only two values (i.e., like and dislike). Assume for a moment that the relationships are reflexive (everyone likes himself) and symmetric (everyone's feelings are reciprocated). Then the Q-space is three-dimensional, with the dimensions representing the three relationships between A and B, B and C, and A and C. This space has exactly 2^3 or 8 points. The schema of balance is a set of exactly four points in this space, namely $(1,1,1)$, $(1-1,-1)$, $(-1,-1,1)$ and $(-1,1,-1)$.

This particular type of Q-space can be applied to the line hypothesis of Abelson and Rosenberg, which forms the basis of what is known as the theory of cognitive balance.[113] Their line hypothesis can be regarded as simply a special case of the specification routine of the present model for the situation in which the selected schema is balance and the distance func-

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$d(x,y) = d(y,x)$; $d(x,y) \geq 0$; $d(x,y) = 0$ if and only if $x = y$, and $d(x,y) + d(y,z) \geq d(x,z)$.

[112] Although the distance between points is a metric function as defined in the previous footnote, the distance between sets is not.

[113] Abelson and Rosenberg, "Symbolic Psycho-logic."

tion is the so-called city block metric (in which the distance between two points is the arithmetic sum of the separate distances separating them on each dimension of the Q-space).

As another example, suppose that the relationships are still reflexive and symmetric but are now continuous (to allow degrees of liking and disliking). The Q-space is still three-dimensional, but now has an infinite number of points. The schema of balance is now the set of points of the pyramid that has for its four corners the four balanced points listed above.[114]

With this geometric background, it is easy to see the implication of changing the size of the maximum distance of a satisfactory fit, k. The smaller the maximum distance for a satisfactory fit, the greater the chance that the more accessible schemata will be rejected and a less accessible (or no) schema will be selected.

In a domain which has precise measurement (such as one of the natural sciences), it is best to set k relatively low. The reason is that if a highly accessible schema gives a mediocre fit in such a situation, it is best to keep searching for a better fit since it is unlikely that random error alone would be responsible for the discrepancy.[115]

Conversely, in a domain which has imprecise information (such as is common in politics) it is best to set k relatively high so that an accessible schema is not rejected when the observed discrepancy could easily have been due to random error.[116]

VII. Illustrative Applications to Politics

Now for a few illustrations to show that the model of how people make sense out of their complex worlds can be directly relevant to political phenomena. These examples are illustrative only, since the range of the potential political applications seems almost to coincide with the range of political science. Because my own interests are primarily in international relations and foreign policy formation, most of the examples are taken from that domain. The reader is invited to see if his favorite political actors try to make sense out of their own complex worlds, and if so whether it makes any difference how they try to do it, and how they might do it better.

Before turning to the illustrations, several points should be made about the operationalization of the schema theory. As a model of what happens in a controlled setting, the schema theory has already been operationalized by the same operations which have been used in the various experiments upon which the theory is based. For example, all of the standard ways of eliciting a subject's beliefs can be used to measure the details of the person's modified and extended specification of a case. As a model of what happens in a political arena, somewhat different measurement techniques are needed. For example, the problems of measuring the beliefs of a political actor are certainly difficult, but hardly unique to this model. Once again, there are a set of standard techniques which can be used to measure beliefs, this time involving the analysis of interviews, questionnaires, documents produced by the actor, and the actor's actual decisions. For the parameters of the model which are not "directly observable," the system level characteristics of the model itself provide alternative indicators that can be used as cross-validating measurement techniques. A good example of the potential for cross-validation is schema accessibility which can be indirectly measured with any of four methods (see III, E).

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[114] Wiest, "A Quantitative Extension."

[115] Signal detectability theory treats the situation in which there is only one accessible schema. See Coombs, Dawes, and Tversky, *Mathematical Psychology*.

[116] Geometric analysis of this section can be extended to yield a number of suggestive theorems about some of the more subtle consequences of the present information processing model. Here are four such results.

(1) *Potential Completeness.* If Q is a continuous space, then under a weak set of assumptions the modified and extended specification of a case will be a complete specification of the case. This corresponds to the situation in which the person can make an estimate about every feature of the case (e.g., every friendship relationship). For example, in Figure 5 the set of points in S_2 which are minimally distant from X is a single point, x', and is therefore a complete specification.

(2) *Potential Enlargement.* Under certain circumstances the modified and extended specification can actually be larger than the initial partial specification. This occurs when there are few points in the partial specification, but more points in the selected schema which are at the minimum distance from the initial partial specification than there were in the initial partial specification itself. In such a situation the person has a schema which he can use to interpret the case, but he does not end up with a single unique specification of the case (even if he started with one). He believes something is wrong with the initial information, but he can not pin down what it is, even though he has selected a schema with which to interpret the case.

(3) *Cancellation Effect.* Under a wide range of conditions, an initial partial specification with continuous parameters will be modified to achieve a fit to the selected schema by leaving most of the parameters unchanged and by giving the rest neutral values. This can be called the cancellation effect since the only changes in the modification are neutralizations. The cancellation effect is a consequence of the principle that the modification is made in such a way as to move the least distance to the selected schema.

(4) *Seriatim Effect.* Under a wide range of circumstances, the sequential presentation of the same information can reduce the amount of attitude change compared to a simultaneous presentation. The seriatim effect is due to the principle that a message is first evaluated for how well it fits into a previous interpretation of the case. Presenting discrepant information a little at a time can result in each message being blamed as it is received, whereas if the discrepant information were presented all at once, the blame might be affixed to the old interpretation.

While waiting for independent evaluations of the model in different political arenas, one can hopefully expect that the literature on perception (and perhaps cognition) will generalize quite well to field studies. People may bargain and exercise power differently in a laboratory than they do in a "real life" situation, but they probably use the same thought processes no matter where they are. This is another way of saying that perception and cognition may be more fundamental psychological processes than bargaining and the exercise of power. Because they may be more fundamental, they probably generalize better from one context to another. But even before the schema theory is empirically evaluated in political contexts, it can serve well heuristically by suggesting new ways of looking at some old political problems. In this spirit the illustrative examples are offered.

A. *Conceptual "Models" of One's Adversaries*

The first example is a direct application of the normative analysis of schema accessibilities (VI, A). As Allison points out, most foreign policy analysts explain and predict the behavior of national governments in terms of a rational policy "model."[117] He also points out that more sophisticated "models," such as an organizational process "model" and a bureaucratic bargaining "model," can be formulated.

If these alternative "models" are treated as schemata which include some possibilities and exclude others, then the present information-processing model can be applied. In particular, the question of which schema should be given highest accessibility can be analyzed. As has been discussed in Part VI, A, there is a tradeoff between improved veridicality of a schema and smallness of size. The improvement of the conditions of the tradeoff comes from improved knowledge of the environment (e.g., by discovering a schema which is both small and veridical, a process that is similar to discovering a theory which is both parsimonious and accurate).

In the meantime (at any given level of knowledge about the environment) the conditions of the tradeoff have this implication: it may be better for the analyst to give higher accessibility to a small but "simple minded" schema (such as one based on the rational policy "model") rather than to a more sophisticated but larger schema.

B. *Individual Belief Systems*

The second example deals with individual belief systems. Like all political actors, a foreign policy maker has his own characteristic belief system which helps shape his perceptions and therefore his decisions. His belief systems can be studied through an analysis of speeches, documents, and interviews. For example, John Foster Dulles followed his usual mode of analysis when he interpreted a large cut in the size of the Russian Army as being due to Russian economic weakness, as possibly leading to an increased production of atomic weapons, and as not lessening world tensions.[118] Thus even a large cut in the Russian Army did not disconfirm Dulles's interpretation of Soviet policy.

Perhaps this is no more than an unusually blatant example of a rather common problem. I conjecture that the schemata people use are often larger than they think, and therefore less easy to disconfirm. When they are actually larger than they think, people are more confident in their own interpretations of past cases and in their prediction of future events than they have a right to be.

C. *Learning Lessons from History*

People often interpret a current case as being analogous to a previous case which was in some ways similar. For example, President Johnson argued that the policy problem of Vietnam was similar to the policy problem of Munich. Historical analogies seem to be drawn by selecting the single previous case which provides the satisficing fit to the present case. This process is therefore a special case of the present model.

The more important to the person a previous case was, the more accessible it will be as an analogy for a future case.[119] Thus "Munich" is a highly accessible analogy, since it is associated with the origins of World War II. This dependence of the accessibility of a prior case on its importance is helpful to the decision maker if (but only if) there is actually a correlation between the importance of a case and the likelihood that it will be a useful guide to future decisions.

D. *The Intelligence Game*

The intelligence/deception interaction is one of the most fascinating aspects of international relations. In terms of the model of how people make sense out of a complex world, three factors make it relatively easy for an intelligent actor (A) to be deceived by another intelligent actor (B) about B's future decisions. First, knowledge and information about the internal decision-making process of B is not well-known to A, and therefore no schema can be both small and veridical. Thus extensions of specifications are not likely to be reliable. Second, actor B can deliberately behave so as to make predictions even harder for A. For example, he can build up a record of consistent behavior and then break the pattern at a crucial moment (which is exactly what the British did to the Germans in the D-Day deception by building up the reliability of double agents under British control.)[120] Third, if actor B knows about the relative accessibility of A's schemata, and if B knows about the type of information available to A on each case, then B can adjust his behavior to increase the likelihood that his behavior will be interpreted by A as fitting a particular schema.

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[117] Graham T. Allison, "Conceptual Models and the Cuban Missile Crisis," *American Political Science Review*, 63 (September, 1969), 689–718.

[118] Ole R. Holsti, "The Belief System and National Images: A Case Study," *Journal of Conflict Resolution*, 6 (September, 1962), 244–252.

[119] Axelrod, *Framework for a General Theory of Cognition and Choice*.

Actor A in trying to predict a future decision by actor B can adopt special tactics to deal with these problems. He can try to avoid being deceived by relying on information which B does not control or which is costly for B to manipulate. Which information has these characteristics, however, is also a matter for A's interpretation, and is therefore subject to the countermeasures by B discussed above. For example, in World War II the Germans trusted the spies they sent to Britain when in fact the British soon controlled every one of them.[121]

E. *The Slow Growth of Knowledge in the Policy Sciences*

In a policy domain each interpretation and decision is important to the actors involved. Because of this, the application of a policy science may develop a set of doctrines. In some cases, such as medicine in traditional societies, these doctrines provide a widely accepted interpretation (diagnosis) and decision in each possible type of case.[122] Such a set of doctrines has three functions. It guarantees that each and every case will get the best interpretation and decision given the current state of the shared knowledge. It promotes confidence on the part of the practitioner and trust on the part of the client, and this confidence and trust can be important in securing results in a policy domain. Finally, the set of doctrines protects the practitioner from charges that he has made a mistake if a particular decision turns out badly.

On the other hand, if the set of doctrines is followed in *every* instance there will be no experimentation and therefore little gathering of information on cases which might disconfirm current theories. For example, in a disease which often has spontaneous remission (such as peptic ulcer), false beliefs about cures can easily arise and be maintained. This is like the dog (mentioned in III, C, 1, a above) who got so good at jumping to avoid a shock that he could not discover when the shock mechanism was turned off.[123]

An exclusive focus on getting the best outcome in each case can therefore retard the growth of discovery of smaller, more veridical schemata. By retarding the growth of knowledge, an exclusive focus on getting the best result in each case can prove a poor tactic in the long run. Therefore, practitioners of policy sciences (such as international relations) should value deliberate experimentation in suitable circumstances. Their clients (such as electorates in democracies) should be willing to tolerate a lower average success in the short run in order to attain a higher level in the long run.

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[120] J. C. Masterman, *The Double-Cross System in the War of 1939 to 1945* (New Haven and London: Yale University Press, 1972).

[121] Masterman, *The Double-Cross System.*

[122] Joseph Ben-David, *The Scientist's Role in Society* (Englewood Cliffs, New Jersey: Prentice-Hall, 1971), pp. 25f.

[123] Solomon, Kamin and Wynne, "Traumatic Avoidance Learning."

Human Rights Without Discrimination

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The Charter of the United Nations requires members to promote human rights "without distinction as to race, sex, language, or religion." In principle, the requirement makes members internationally accountable for policies traditionally regarded as purely domestic. More than that, it offers a basis for the global development of standards for the equal and just treatment of individuals, and thus a basis for action contributing to the formation of a community of mankind.

So far, the record of the members is quite mixed with respect to action at the international level to promote implementation of the requirement. The General Assembly has adopted a number of declarations, conventions, and covenants spelling out human rights, thus reducing the vagueness of the Charter provision; but the record of members in ratifying the conventions and covenants is hesitant, the United States ratifying very few.[1] All of the principal organs of the United Nations as well as many of the associated independent agencies have been or are involved in the effort to obtain greater respect for human rights in Southern Rhodesia, South Africa, and Namibia. But the preoccupation with violations of human rights in these areas is accompanied by substantial neglect of violations elsewhere.

Nevertheless the fact that implementing action occurs means that the requirement has significance; and especially in the light of increasing concern through much of the world with ethnic and racial problems and with the status of women, an analysis of its meaning and potentialities is in order. That is what will be attempted here. The first question is whether "without distinction" means what it seems to say, that is, whether it means "without any differentiation." If it does mean this, then separate toilet facilities for men and women apparently violate the obligation, and so do separate sleeping arrangements for male and female prisoners. If "without distinction" does not mean what it seems to say, then what does it mean? The second question is whether the naming of race, sex, language, and religion means that "distinction" on other bases remains permissible.

The answer to these questions turns out to be that "without distinction" means "without discrimination," and that what is ruled out is discrimination *of any kind* that affects human rights. And thus the third question concerns the implications of an international prohibition of discrimination. More broadly, the third question is whether the search for equality has not been internationalized, at least in principle, and what the implications of the internationalization might be.

"Without Distinction" and "Without Discrimination"

According to one of the common meanings, to make a distinction is to distinguish or to differentiate. It is thus natural to suppose that the "without distinction" requirement of the Charter means "without differentiation": members are to promote human rights "without differentiation as to race, sex, language, or religion." But the adoption of this meaning leads quickly to implications and consequences that could scarcely have been intended. Many kinds of differentiations on the bases named are commonly regarded as quite acceptable, if not desirable and necessary. The United Nations itself makes distinctions as to language, selecting "official" and "working" languages, and the choice entails relative advantage to some countries and delegations and disadvantage to others. The United Nations Commission on Human Rights has a Sub-Commission on Prevention of Discrimination and Protection of Minorities, which implies that, while "discrimination" is to be prevented, minorities may be distinguished and protected. When the General Assembly recommended the partition of Palestine, it surely intended to make distinctions as to language and religion, and the carving of Pakistan out of colonial India surely also reflected a distinction as to religion. Thus the very *raison d'être* of some states, or of the location of specific international boundaries, would be contradicted if "without distinction" were interpreted to mean "without differentiation." Moreover, such an interpretation would presumably rule out reverse or benevolent discrimination based on race or sex. And it would presumably mean that in multilingual communities, children could not be assigned to school on the basis of distinctions as to language.

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[1] Among the instruments adopted by the General Assembly are the Universal Declaration of Human Rights (1948), the Genocide Convention (1948), the Convention on the Elimination of All Forms of Racial Discrimination (1965), the Covenant on Civil and Political Rights (1966), and the Covenant on Economic, Social, and Cultural Rights (1966). The United States has not ratified any of these conventions or covenants. It has ratified a Supplementary Slavery Convention. On the policies of the United States, see Vernon Van Dyke, *Human Rights, the United States, and World Community* (New York: Oxford University Press, 1970), pp. 129–156.

If "without distinction" can scarcely mean "without differentiation," then what does it mean? A clue is provided in the name of the Sub-Commission referred to above: the Sub-Commission on Prevention of Discrimination and Protection of Minorities. What is to be prevented is not *distinction* in the sense of differentiation, but *discrimination*. In fact, in many international instruments the terms *distinction* and *discrimination* are used indiscriminately. The Universal Declaration of Human Rights says "without distinction" in Article 2 and "without any discrimination" in Article 7.[2] The Covenant on Civil and Political Rights says "without distinction" in Article 2, and the comparable article in the Covenant on Economic, Social and Cultural Rights says "without discrimination." The Declaration on the Rights of the Child says "without distinction or discrimination."

A comparison of usage in English and in French yields further clues. The Treaty of Versailles called for a treaty between Poland and the Free City of Danzig which, among other things, was "to provide against any discrimination within the Free City of Danzig to the detriment of citizens of Poland and other persons of Polish origin or speech." The French version renders "any discrimination" as "*aucune discrimination*," but then the treaty that Poland and Danzig concluded (like all the minority treaties after World War I) uses the expression "without distinction" in English and "*sans distinction*" in French. The European Convention on Human Rights, signed in 1950, says "without discrimination" in the English version and "*sans distinction aucune*" in the French (Article 14). The Covenant on Economic, Social, and Cultural Rights, mentioned above, says "without discrimination" in English and "*sans distinction injuste*" in French.

Confusion about "distinction" and "discrimination" no doubt arises partly because *distinction* is the traditional French word, *discrimination* being a relatively recent addition to the French language.[3] The French Declaration of the Rights of Man said "*sans distinction,*" and not "*sans discrimination.*" It proclaimed the principle that all should be equal in the eyes of the law and equally eligible to all honors and governmental positions, according to their ability, "*sans autre distinction que celle de leurs vertus et de leurs talents.*" The *Grand dictionnaire universel du XIX^e siècle* (Larousse, 1870) does not contain the word *discrimination*. The *Larousse du XX^e siècle* (1929) includes the word, but defines it simply as "capacity to discern, to distinguish," whereas it illustrates the meaning of *distinction* with the statement that justice shall be administered "*sans distinction.*" Similarly, the *Dictionnaire de l'Académie française* (1932) includes *discrimination*, defining it simply as "*action de distinguer avec precision.*" The *Grand Larousse encyclopédique* (1961) offers approximately the same definition, and then adds "*discrimination raciale,*" referring the reader to *racisme* and *ségrégation*. Only with the *Robert Dictionnaire alphabétique* (1967) does *discrimination* appear with a general meaning that is relevant here: separate and adverse treatment.

Presumably the use of *distinction* in French has influenced the choice of words in English, and now the use of *discrimination* in English is having a comparable influence on the choice of words in French. Everything suggests that *distinction* and *discrimination* have become synonyms. René Cassin took this view in 1948, assuring the United Nations Commission on Human Rights that "there was no difference of substance between the words 'distinction' and 'discrimination' in French."[4] In 1962, when misgivings were expressed in the Third Committee of the General Assembly about the word *distinction*, an understanding was informally reached that it should not be construed to prohibit "the adoption of legitimate measures designed precisely to safeguard and promote the rights of certain categories of persons." In particular, "protective measures taken by various States for the benefit of certain socially and educationally backward groups of the population were aimed at re-establishing equality and could not therefore be violations [of the 'without distinction' requirement]."[5] By general consent, then, the requirement can be read as if the reference is to discrimination.[6]

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[2] The Universal Declaration and most of the other international documents on human rights cited herein are conveniently available in *Basic Documents on Human Rights,* ed. Ian Brownlie (Oxford: Clarendon, 1971).

[3] Cf. European Court of Human Rights, Series B: Pleadings, Oral Arguments, and Documents. Case "Relating to Certain Aspects of the Laws on the Use of Languages in Education in Belgium" (Strasbourg, 1967), Vol. II, pp. 79–80. (Hereafter cited as Eur. Court H. R., Series B, "Linguistic" Case.)

[4] E/CN.4/SR.53.

[5] A/5365, paras. 57–58.

Both the European Commission of Human Rights and the European Court of Human Rights take this same view in interpreting the European Convention, where (as indicated above) "without discrimination" in English becomes "*sans distinction aucune*" in French. Both denied that *discrimination* and *distinction* have different meanings in the given context. More specifically, they declared that it would lead to "absurd results" if "*sans distinction*" were interpreted to preclude differentiation, and they pointed to the many distinctions that are in fact accepted without being considered discriminatory, some of them designed to promote equality rather than to deny it.[7] It is generally agreed, for example, that in multilingual communities it is permissible to distinguish between children by language so as to provide mother-tongue instruction.

"As to Race, Sex, Language, or Religion"

Now the question is whether the naming of race, sex, language, and religion means that discrimination is permissible as long as it occurs on other grounds. This question is perhaps not as significant as the first, since the grounds named are the ones that arouse the most objection. Further the answer cannot be quite as confident. Nevertheless, the question is important enough to justify a brief inquiry. Its importance is suggested by the increased attention given to economic factors in the United States in connection with the equal protection of the laws. Thus impecunious defendants are entitled to various forms of public assistance; and the question exists whether school children are not denied equal protection of the laws if their education depends on a tax base that differs considerably in various school districts.

International instruments other than the Charter differ in the breadth of their attack on discrimination. Some are narrower than the Charter, focusing, for example, on discrimination based on race or on sex. Others are broader. The Universal Declaration of Human Rights, adopted by the General Assembly three years after the Charter came into effect, says "without distinction *of any kind* [my italics], such as race, colour, sex, language, religion, political or other opinion, national or social origin, property, birth or other status." Precisely the same words appear in the two Covenants, except that the Covenant on Economic, Social, and Cultural Rights (as already indicated) says "without discrimination" instead of "without distinction." In addition to the phrase quoted, the Universal Declaration goes on to say that "all are equal before the law and are entitled without any discrimination to equal protection of the law." Virtually the same words appear in the Covenant on Civil and Political Rights (Article 26), and it adds that "the law shall prohibit any discrimination and guarantee to all persons equal and effective protection against discrimination *on any ground* [my italics]. . . ."

Domestic constitutions and laws also differ in the breadth of their attack on discrimination. Some are like the Charter in ruling out a few potential grounds of discrimination; race and religion are more commonly named than sex and language. Others are like the Universal Declaration and the Covenants, ruling out discrimination in general or providing affirmatively for the equal protection of the law. The constitution of the United States is, of course, in the broader category.

The issue, then, is open to argument. Should lists of forbidden grounds of discrimination be presumed to be exhaustive? The best argument is in the negative. With respect to the Charter, the argument is that nondiscrimination is implied in the obligation to promote human rights "for all," and that the "without distinction" phrase is simply a partial and nonrestrictive elaboration of a meaning that is there in any case. This is borne out in the wording of the Universal Declaration, which regularly refers to "all," to "everyone," and to "no one." The Covenants contain similar language. Moreover, many of the domestic constitutions that list forbidden grounds of discrimination also provide affirmatively for equality or the equal protection of the laws, which suggests that the list was not intended to be exhaustive or restrictive. When such lists are made, as in the Charter, what apparently happens is that those grounds get included that are currently arousing the most resentment or objection; the purpose in naming them is to make it doubly sure that they are outlawed. But there is no reason to suppose that the authors of the lists intended them to be exhaustive; and even if there were doubt on this point, those who come along later are reasonably free to choose among the permissible interpretations, adopting the one that contributes most constructively to a desirable social and political order, domestic and global.

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[6] Cf. W. A. McKean, "The Meaning of Discrimination in International and Municipal Law," *British Yearbook of International Law*, Vol. 44 (1970), 180.

[7] Eur. Court H. R., Series B, "Linguistic" Case, Vol. II, 80. European Court of Human Rights. Series A: Judgments and Decisions, 1968. Case "Relating to Certain Aspects of the Laws on the Use of Languages in Education in Belgium" (Merits). Judgment of 23 July 1968 (Strasbourg, 1968), p. 34. (Hereafter cited as Eur. Court H. R., Series A, "Linguistic" Case, Judgment of 23 July 1968.)

What Constitutes Discrimination?

If human rights are to be promoted without discrimination on any ground, it is important to know what constitutes discrimination. A thorough answer would be broad and complex, for relevant thought has many aspects. The question has been much explored already. What will be attempted here is a sketch of an answer, oriented toward developments at the international level. The main object is to indicate how significant the rule of nondiscrimination potentially is for the development of a community of mankind. Whether the potentiality is ever realized is another matter.

Negative and Positive Claims to Equality. It is essential to relate the idea of discrimination to the idea of equality, and helpful to think of claims to equality as either negative or positive. Claims to nondiscrimination are negative claims to equality. It is the negative claims that have been stressed in the western tradition. Thus Benn and Peters say,

> The demand for equality has never been intended in practice as a general plea that all men be treated alike. Egalitarians have always been concerned to deny the legitimacy of certain sorts of discrimination resting on some given differences, i.e., they have challenged established criteria as unreasonable, and irrelevant to the purposes for which they were employed. Claims to equality are thus, in a sense, always negative, denying the propriety of certain existing inequalities.[8]

If claims to equality are "always negative" in the western countries, it is not surprising that negative expressions of the claim would be prominent at the international level. But affirmative claims are also recognized. Obligations are commonly qualified so as to permit affirmative action on behalf of equality, whether the action be protective, compensatory, or corrective; and sometimes affirmative action is required. In truth, both the negative and the positive aspects of the matter show up in the Charter itself, the "without distinction" requirement being the negative and the obligation to "promote" human rights, which include the equal protection of the laws, being positive.

The dual aspect of the matter shows up in an opinion of the Permanent Court of International Justice in 1935 in a case concerning the right of the Greek minority community in Albania to have its own schools. Advising on the matter the Court pointed to the possibility that nominal equality in law might result in inequality in fact. "Equality in law," it said, "precludes discrimination of any kind; whereas equality in fact may involve the necessity of different treatment in order to attain a result which establishes an equilibrium between different situations."[9] The relevant point here is that "different treatment" is not necessarily discriminatory; it may be a response to a positive claim to equality.

The negative and positive approaches to equality show up in many other connections. They are suggested by the very name of a subcommission of the UN Commission on Human Rights: the Sub-Commission on Prevention of Discrimination and Protection of Minorities. In 1947 the Sub-Commission adopted statements defining key words in its name:

> 1. Prevention of discrimination is the prevention of any action which denies to individuals or groups of people equality of treatment which they may wish.
> 2. Protection of minorities is the protection of non-dominant groups which, while wishing in general for equality of treatment with the majority, wish for a measure of differential treatment in order to preserve basic characteristics which . . . distinguish them from the majority of the population.[10]

The two approaches appear in many other connections. In the case of indigenous peoples, it is a long-established rule that no discrimination against them shall occur but that, on the contrary, special measures for their protection and advancement are at least permissible if not required. In 1962 the Sub-Commission on Prevention of Discrimination and Protection of Minorities formulated a set of "General Principles on Freedom and Non-Discrimination in the Matter of Political Rights."[11] It reaffirmed the "without distinction" rule, went on to affirm various political rights, and then listed "measures which shall not be considered discriminatory." Among the latter are

> special measures to ensure: (i) the adequate representation of an element of the population of a

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[8] S. I. Benn and R. S. Peters, *The Principles of Political Thought* (New York: Free Press, 1965), pp. 131–132.

[9] Permanent Court of International Justice. XXXIVth Session. 1935. Series A./B. Judgments, Orders, and Advisory Opinions. Advisory Opinion of April 6th, 1935. Fascicule No. 64. Minority Schools in Albania, p. 19.

[10] United Nations. Commission on Human Rights. Sub-Commission on Prevention of Discrimination and Protection of Minorities. *The Main Types and Causes of Discrimination* (1949.XIV.3), p. 2.

[11] United Nations. *Study of Discrimination in the Matter of Political Rights*, by Hernán Santa Cruz (New York, 1962; 63.XIV.2), pp. 96–99.

country whose members are in fact prevented by political, economic, religious, social, historical, or cultural conditions from enjoying equality with the rest of the population in the matter of political rights; (ii) the balanced representation of the different elements of the population of a country; provided that such measures are continued only so long as there is need for them, and only to the extent that they are necessary.

Illustrations of the negative and positive approaches to equality appear in various international conventions, for example, the Discrimination (Employment and Occupation) Convention (1958), sponsored by the ILO, the Convention Against Discrimination in Education (1960), sponsored by UNESCO, and the Convention on the Elimination of All Forms of Racial Discrimination (1965), sponsored by the United Nations. Discrimination is defined in approximately the same way in all three. The wording in the Convention Against Discrimination in Education is as follows:

> The term 'discrimination" includes any distinction, exclusion, limitation or preference which, being based on race, colour, sex, language, religion, political or other opinion, national or social origin, economic condition or birth, has the purpose or effect of nullifying or impairing equality of treatment.[12]

Then comes the positive undertaking. Parties bind themselves affirmatively "to promote equality of opportunity and of treatment in the matter of education" in various specified ways. The Discrimination (Employment and Occupation) Convention likewise accompanies the ban on discrimination with the assertion of an affirmative obligation "to promote . . . equality of opportunity and treatment" and with a dispensation permitting "special measures of protection or assistance" for certain kinds of groups. The Convention on the Elimination of All Forms of Racial Discrimination includes comparable provisions, negatively banning discrimination and positively requiring promotional efforts.

> States Parties shall, when the circumstances so warrant, take, in the social economic, cultural and other fields, special and concrete measures to ensure the adequate development and protection of certain racial groups or individuals belonging to them, for the purpose of guaranteeing them the full and equal enjoyment of human rights and fundamental freedoms.

The Individual and the Group. In the western tradition the rights of man are thought of as individual rights, and formally the spelling out of human rights is the spelling out of rights for individuals. The very notion of human rights "without distinction as to race, sex, language or religion" suggests an atomistic world in which the individual is the important unit, in which groups scarcely count, and in which differentiation among individuals on the basis of their membership in groups is suspect if not forbidden. Paradoxically, however, the greater concern for human rights is associated with a greater consciousness of kind. It is as if the individual can gain pride and dignity only through identification with a group whose status and role command respect. Even the agitation against discrimination based on sex is associated with signs of an affirmation of sexual solidarity.

Whatever the reason, negative protests against discrimination are counterpoised with positive demands relating largely to groups. The fact is illustrated most generally by the demand, which welled up outside the imperial countries, that self-determination be named as a human right. To a considerable extent, this was a demand that a major manifestation of racial discrimination should end. Beyond this, demands concerning racial, ethnic, linguistic, and religious groups show up almost everywhere in the world. Group struggles occur over linguistic problems, for example, in Belgium, Canada, Ceylon, and India. Canada has gone bilingual. Belgium has opted for a division into strictly unilingual zones, the claim being that bilingualism would "debase the traditional character and individuality of the two communities."[13] The Cabinet in Belgium must consist of an equal number of French-speaking and Dutch-speaking ministers.[14] Ceylon has swung to "Sinhala only," to the discomfiture of its Tamil minority. India has created new states on a linguistic basis, but problems remain acute.

Religious groups as such no longer play the role in politics that they once did, but they have not gone completely offstage. India's positive efforts to promote equality include preferential treatment for the Scheduled Castes—as well as for the Scheduled Tribes and the "backward." The very creation of Pakistan reflected a religious division, as did, in a sense, the creation of Israel. Lebanon allocates seats in its parliament and many other governmental positions to religious groups as such. Cyprus, divided into Turkish and Greek communities identified by both language and religion, follows a similar plan.

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[12] The convention makes certain exceptions explicit. "Separate but equal" education by sex is permissible; and separate educational systems established in keeping with the wishes of parents for religious or linguistic reasons are also permissible, if enrollment is optional and if appropriate standards are maintained.

[13] Robert Senelle, "The Revision of the Constitution 1967–1971," *Memo from Belgium,* Nos. 144–145–146, January-February-March 1972 (Brussels: Ministry of Foreign Affairs, External Trade and Cooperation in Development), p. 15.

[14] Senelle, p. 186. Article 86b of the constitution.

Ethnic politics is nearly universal. For example, in Somalia "the formula for establishing a new government was . . . found in clan balance, the establishment of an equilibrium of the major ethnic groups . . . "[15] The Malay constitution assures the Malays a "special position" in public and economic life. New Zealand assures the Maoris of a quota of four seats in parliament.

Fiji got independence only when an elaborate arrangement was worked out to reassure the various ethnic groups. Voters register on one of three rolls, depending on whether they are Fijian, Indian, or "other," and the three rolls together constitute a national roll. The Fijis, Indians, and "others" are respectively assigned 22, 22, and 8 members of the House of Representatives. Of the 22, 12 are elected in each case by those on the appropriate ethnic roll and 10 by those on the national roll; of the 8, the corresponding division is 3 and 5.[16]

And so it goes. These illustrations of differentiations between groups, and of the assignment of rights to groups, are among the more outstanding ones, but many others exist.

Discrimination as Unreasonable Differentiation. Given the combination of a rule against discrimination and a principle that "special measures" on behalf of various kinds of groups are permissible if not required, the classification of specific policies as discriminatory or egalitarian becomes a matter of judgment. No litmus paper, no strict rule, provides a generally applicable, objective test.

This position contrasts so sharply with that taken before the International Court of Justice in the South West Africa Cases in 1964 that the contrast deserves to be pointed out. In those Cases the Applicants claimed that "a generally accepted international human rights norm of non-discrimination or non-separation" now exists. They defined "non-discrimination" or "non-separation" as follows:

> stated negatively, the terms refer to the absence of governmental policies or actions which allot status, rights, duties, privileges or burdens on the basis of membership in a group, class or race rather than on the basis of individual merit, capacity or potential: stated affirmatively, the terms refer to governmental policies and actions the objective of which is to protect equality of opportunity and equal protection of the laws to individual persons as such.[17]

They insisted "that the allotment, by governmental policy and action, of rights and burdens on the basis of membership in a 'group,' irrespective of individual quality or capacity, is impermissible discrimination, outlawed by legal norms well established in the international community." Earlier in the proceedings they had asked the Court to pronounce that South Africa's policies of *apartheid* in South West Africa were "arbitrary, unreasonable, unjust and detrimental to human dignity,"[18] which would have called for an exercise of judgment about the effects of the policies, but they dropped this request in favor of the stricter and more nearly automatic rule. Pressed about that rule, they later wavered, admitting that "protective" measures for groups were sometimes justified, but never providing a principle for reconciling the ideas of nondiscrimination and nonseparation with the idea of protection. The Court dismissed the case without ruling on the merits. Later, as will be noted in a moment, it declared that South Africa's policies violate its international obligations, but it did not subscribe to the more sweeping and general claim that any official allotment of rights or burdens on the basis of membership in a group is impermissible.

In a dissenting opinion in the South West Africa Cases, Justice Kotaro Tanaka struck the right chord when he said,

> Different treatment is permitted when it can be justified by the criterion of justice. One may replace justice by the concept of reasonableness generally referred to by the Anglo-American school of law. Justice or reasonableness as a criterion for the different treatment logically excludes arbitrariness. . . . We must recognize . . . the legality of different treatment so far as justice or reasonableness exists in it.[19]

The same chord, along with some others, was repeatedly struck in the Belgian linguistic case considered by the European Commission of Human Rights. The case concerned alleged discrimination in Belgian policies directed at the language problem. No one contended that "distinction" in the sense of differentiation was proscribed. The question was whether the differentiations were reasonable or whether they were unfair or unjust or arbitrary. "Discrimination," said the Commission, "must not be confused with legitimate distinctions designed to re-establish rather than to destroy equality."[20] The Commission even took the view that especially favorable treatment for one group did not necessarily constitute discrimination; there also had to be correlated disadvantage or hardship for another group; and on top of that the disadvantage or hardship "should not seem justified by administrative, financial or other needs or, more generally, by considerations based on the general interest or the common good."[21] The Commission referred to Justice Tanaka's criterion of reasonableness, and said that its own criterion was "not essentially different."[22] The Counsel for the Belgian government took the view that in deciding whether legislation was discriminatory, one should ask whether it "is in accord with the general interest, whether or not there are valid motives for it"; one should ask whether actions are "arbitrary" or whether they can be "justified."[23] The Court itself took approximately the same line. Its question was whether the distinctions that Belgium was making had an "objective and reasonable justification," and whether there was a "reasonable relationship of proportionality between the means employed and the aim sought."[24] The Court ruled that the provision of the European convention on human rights

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[15] A. A. Castagno, Jr., "Somali Republic," in James S. Coleman and Carl G. Rosberg, Jr., eds., *Political Parties and National Integration in Tropical Africa* (Berkeley: University of California Press, 1964), pp. 546–7.

[16] *The Fiji Independence Order 1970* (London: HMSO), p. 35.

[17] International Court of Justice. Pleadings, Oral Arguments, Documents. "South West Africa Cases" (1966), 4, 492–493.

[18] International Court of Justice. Pleadings, Oral Arguments, Documents. "South West Africa Cases" (1966), 1, 20–21.

[19] International Court of Justice. *Reports of Judgments, Advisory Opinions and Orders, 1966,* pp. 306, 309.

> does not prohibit distinctions in treatment which are founded on an objective assessment of essentially different factual circumstances and which, being based on the public interest, strike a fair balance between the protection of the interests of the community and respect for the rights and freedoms safeguarded by the Convention.[25]

As Justice Tanaka's reference to "the Anglo-American school of law" suggests, the doctrine of reasonableness brings the international problem of defining discrimination within the framework of the domestic argument over the equal protection of the laws. That doctrine is obviously troublesome, and the troubles are not much alleviated by the use of such supplementary words as *unjust, unfair,* and *arbitrary.* Experience in American courts speaks eloquently on the point. Nor are the troubles reduced when consensus about reasonableness is sought not simply within the United States but over the world with its endless variety. This is suggested by the fact that the Counsel for the government of South Africa, defending *apartheid* before the International Court of Justice, had no objection at all to the "common attitude . . . that differentiation of the nature of unfair discrimination is impermissible but that measures of differentiation which are genuinely designed to promote the interests of all the groups concerned, is permissible."[26] His claim was that *apartheid* is fair and in the interests of all the groups concerned. The same kind of attitude shows up in various Rhodesian constitutions and in the "Proposals for Settlement" which Britain and Rhodesia worked out in 1971. After calling for a prohibition of discrimination, the "Proposals" went on to say that a law would not be considered discriminatory if it "is reasonably justifiable either in the interests of Rhodesia as a whole or in order to secure the protection, in an equitable manner as between the [groups] affected, of their respective interests."[27] But if disagreement about what is reasonable, just, and fair is inevitable, the words at least fix a common basis and framework for debate. Moreover, the record shows that some degree of international agreement is possible. The European Court of Human Rights found it possible to render judgment in the Belgian linguistic case. Further, as noted above, the International Court of Justice, in a 1971 advisory opinion, after noting that South Africa, like all members of the United Nations, is obliged to promote human rights for all without distinction as to race, went on to declare:

> To establish instead, and to enforce, distinctions, exclusions, restrictions and limitations exclusively based on grounds of race, colour, descent or national or ethnic origin which constitute a denial of fundamental human rights is a flagrant violation of the purposes and principles of the Charter.[28]

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[20] Eur. Court H. R. Series B, "Linguistic" Case I, 212.

[21] Eur. Court H. R., Series B, "Linguistic" Case, II, 47.

[22] Eur. Court H. R., Series B, "Linguistic" Case, II, 82–83.

[23] Eur. Court H. R., Series B, "Linguistic" Case, II, 151.

[24] Eur. Court H. R., Series A, "Linguistic" Case, Judgment of 23 July 1968, p. 34.

[25] Eur. Court H. R., Series A, "Linguistic" Case, Judgment of 23 July 1968, p. 44.

[26] International Court of Justice. *Pleadings, Oral Arguments, Documents.* "South West Africa Cases" (1966), VIII, 699.

[27] Great Britain. Parliamentary Papers. *Rhodesia. Proposals for a Settlement.* November 1971. Cmnd. 4835, p. 36.

[28] Text of opinion in *American Journal of International Law,* 66 (January, 1972), 180–181. Cf. Egon Schwelb, "The International Court of Justice and the Human Rights Clauses of the Charter," *American Journal of International Law,* 66 (April, 1972), 348–349.

It is notable enough that in the Charter the members accept an international obligation with respect to a matter that, prior to the Charter, was within their domestic jurisdiction. And it is almost equally notable that a start has been made toward consensus on criteria for interpreting the obligation.

Dangers and Potentialities

Obvious danger attends positive efforts to promote equality when the efforts are directed toward racial, ethnic, linguistic, or religious groups as such, for, as the Convention on racial discrimination says, they might "lead to the maintenance of separate rights" for different groups. Instead of contributing to the development of a community of mankind, they might accentuate its divisions, sustaining primordial loyalties.

At the same time, the internationalization of the search for equality has startling potential implications. The very fact of internationalization denies that the sovereign state is the ultimate unit within which questions of equality are to be considered. The unit is now mankind. Comparisons that establish the fact of discrimination or equality are to be made on a global basis. Joint efforts—collective efforts—to promote human rights are required. The Universal Declaration of Human Rights says that "everyone is entitled to a social and international order in which the rights and freedoms set forth in this Declaration can be fully realized." The constitution of the International Labor Organization says that it must be "the central aim of national and international policy [to attain] conditions of freedom and dignity, of economic security and equal opportunity [in which] all human beings, irrespective of race, creed, or sex [can] pursue both their material well-being and their spiritual development." If within the United States a child is denied equal protection of the laws when his education depends on the local property tax, then throughout the world there may be an equivalent denial when education depends on purely national resources. If it is discriminatory within the United States to permit some to make TV broadcasts while excluding others, then principles that have been accepted at the international level make it discriminatory in the Soviet Union too. The internationally endorsed principles are obviously visionary and Utopian, but thought is in order about the direction mankind is taking and about the nature of the world community a century or two hence.

Minority Electoral Politics in a North Indian State: Aggregate Data Analysis and the Muslim Community in Bihar, 1952–1972*

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The practice of electoral democracy places a hard dilemma before minority groups. If the members of the minority want to participate as fully equal members of the polity, they must integrate themselves into the larger group and play the games of politics according to the majority's rules, but they do so at the risk of seeing their minority identity and culture disappear. If on the other hand they insist on retaining their solidarity and group identity, they must act as a cohesive unit, a tactic which will underline their separateness from the larger society but at the risk of continued isolation and political impotence for the group. The French Huguenots in the United States might be said to be a prototype of the first kind of minority, while the Amish of Pennsylvania could serve as an extreme example of the second.

For most minority groups the two strategies amount to unrealistic and undesirable endpoints on a continuum, and in practice both are pursued at the same time in diluted form: the group adopts enough of the behavioral practices of the larger polity to participate successfully in the political process, while at the same time retaining its own identity and in fact continuing to behave as a distinct entity in the political process. Such at least have been the findings of most of the recent literature on minority group politics in the United States.[1]

Some groups are geographically concentrated and have found it possible to exercise local domination through the political process, although often at the cost of equality in the political system as a whole. An example would be the French in Canada, who for many decades appeared quite content to dominate Quebec, especially in matters of language, education and religion, even though they did so at the price of second class status in the rest of the Dominion.[2]

Minority groups that are geographically dispersed, however, cannot pursue such a strategy, for they have no significant areas of local control into which the collective ego may retreat. Blacks in America are an obvious case in point. Only in a few rural counties in the deep South and in crumbling, ghettoized cities of the North do they have a majority of the population; everywhere else they are only a small portion of the body politic.

In British India there were a number of areas of the first type, where Muslims were dominant, even though the subcontinent as a whole had an overwhelming majority of Hindus. The Partition in 1947, however, insured that the Muslims remaining in the Indian Union would be a minority of the second kind, for virtually all the Muslim-dominant areas went to Pakistan in the Radcliffe award that determined the boundaries of the new countries.[3] During the constituent assembly deliberations that began

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[1] See, for example, Edgar Litt, *Ethnic Politics in America: Beyond Pluralism* (Glenview, Ill.: Scott, Foresman, 1970), and Nathan Glazer and Daniel Patrick Moynihan, *Beyond the Melting Pot: The Negroes, Puerto Ricans, Jews, Italians and Irish of New York City*, 2nd. ed. (Cambridge, Mass.: MIT Press, 1970); also the volumes of readings edited by Brett W. Hawkins and Robert A. Lorinskas, *The Ethnic Factor in American Politics* and by Harry A. Bailey, Jr., and Ellis Katz, *Ethnic Group Politics* (both Columbus, Ohio: Charles E. Merrill, 1970 and 1969 respectively). Historically and comparatively in multiethnic societies, there have been a number of other situations beside those noted here; see Philip Mason's encyclopedic study *Patterns of Dominance* and also his *Race Relations* (both London: Oxford University Press, 1970).

[2] See D. Kwavnick, "The Roots of French-Canadian Discontent," *Canadian Journal of Economics and Political Science*, 31 (November, 1965), 509–523. On the strategies of various French Canadian political leaders, see Michael Oliver, "The Strategy of French Canadian Participation in Canadian Politics," a paper presented at the International Political Science Association Conference, Ann Arbor, Michigan, September 1960.

[3] Kashmir has a population that is more than two-thirds Muslim, but its internal politics have been of a rather special character that does not typify the rest of the country. Otherwise Assam (as constituted in 1961) has the highest proportion of Muslims (23.3 per cent) of any of the states. As to districts, only Murshidabad in West Bengal and Malappuram in Kerala have a majority of Muslims. In a sense there did remain for a short time after Partition in 1947 an area of Muslim dominance, for the "communally reserved" constituencies set up under the Government of India Act of 1935 provided legislative representatives for the Muslim community. India continued to be governed under this act until the new Constitution came into effect in 1950.

after independence, it was thought by some that minority religious groups should be allotted legislative seats on a quota basis, but in the end it was decided that all groups would compete for the same seats, and the first general elections of 1951–52 were conducted on that basis.[4] Since then the Muslims have had to compete with the majority Hindu community on the latter's own electoral ground.

This paper employs an aggregate data analysis based on mapping to examine that competition in the north Indian state of Bihar, a unit of about 56 million people in 1971 (roughly the same in population as France) and approximately 67,000 square miles (about the size of the state of Washington). First some consideration will be given to the problems involved in doing aggregate data analysis with the Indian data and the advantages and deficiencies of various methods of ecological analysis thus far evolved to cope with these problems. The method to be used in this paper will then be presented, and an analysis of Muslim representation and participation in six Bihar Legislative Assembly elections from 1952 to 1972 will be undertaken.

Aggregate Data Analysis and Indian Politics

It seems at first glance strange that there has been thus far relatively little aggregate data analysis of Indian elections. The two major requisites for such work—voting returns and socioeconomic data—are available in abundance. There have been five national elections with profuse accompanying numerical data; and the Indian Census of 1961, which will run to more than a thousand volumes when completed, holds tantalizing prospects of almost endless and rewarding research to the student of politics interested in ecological analysis. The abundant statistical coverage at all levels from state down to village may be compared with data from the five elections. For India such an opportunity has a particular appeal, because survey data are expensive, methodologically and administratively difficult to obtain in such a huge country, and therefore exceedingly rare.[5] In spite of the resurgence of interest in "ecological" political analysis generally in recent years,[6] though, the promised Indian feast of aggregate data analysis has for the most part proven lamentably meager.

There are two major reasons for the failure to exploit this resource. One is the political integrity of the Election Commission, which has insisted on redrawing the constituency boundaries after every census, so as to make representation as equitable as possible. The practice most certainly buttresses the structure of parliamentary democracy in India, but it does make life frustrating for the political scientist, who consequently cannot compare elections across redrawn constituencies without a great deal of trouble.

The other problem is the general lack of geographical fit between census tracts and electoral constituencies.[7] The census units are almost invariably the administrative units of revenue village, police *thana/tehsil,* district, and so on; the constituencies, on the other hand, are drawn to ensure equal populations, and wander over administrative boundary lines with a fine contempt for the scholar's convenience.

A number of schemes have been devised in recent years to overcome this problem of mismatching census and constituency boundaries, all in varying ways unsatisfactory. One method is to rectify the political units with the census units, and this is the approach that has been employed most often. With seats for Members of the Legislative Assemblies of the Indian states (MLA's) it is easy to aggregate to the district level, for in only a very few of the 300-plus districts in India do political boundaries cross district lines.[8] The continuity of district boundaries over time lends added attractiveness to the use of the district level, making possible the comparison of results for all elections, not just those held in the periods between constituency redelineations. The existing socioeconomic data are much richer for the districts than for subordinate units; published data by district are readily available on agricultural output, acreage under different crops, per capita income, industrial undertakings and electricity use, but are available either not at all or only with great difficulty for smaller units.[9]

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[4] On the decision to eliminate reserved seats for Muslims, see Granville Austin, *The Indian Constitution: Cornerstone of a Nation* (Oxford: Clarendon Press, 1966), pp. 144–156. Reserved seats were retained for the Scheduled Castes (Harijans, or Untouchables) and Scheduled Tribes.

[5] There are exceptions, unfortunately thus far few. The major effort to date has been the work of the Centre for the Study of Developing Societies in New Delhi and that of the opinion pollster E. P. W. da Costa. Some of the findings of the Centre's 1967 election survey have been published in *Asian Survey*'s symposium issue on "Elections and Party Politics in India," 10 (November, 1970). Ramashray Roy of the Centre analyzes a survey done for the 1969 election in his *The Uncertain Verdict* (Bombay: Orient Longmans, 1972). As an example of da Costa's work, see "The Indian General Elections 1967, the Structure of Indian Voting Intentions: January 1967, A Gallup Report with Analysis" (New Delhi: Indian Institute of Public Opinion, 1967).

[6] See for instance Mattei Dogan and Stein Rokkan, eds., *Quantitative Ecological Analysis in the Social Sciences* (Cambridge: M.I.T. Press, 1969), and Richard L. Merritt and Stein Rokkan, eds., *Comparing Nations: The Use of Quantitative Data in Cross-National Research* (New Haven: Yale University Press, 1966).

[7] R. Chandidas examines the problem in considerable detail in "Ecological Correlates of Politics: Problems of Data Equivalence," a monograph presented to the Data Confrontation Seminar held at the Inter-University Consortium for Political Research, University of Michigan, 1969 (mimeo.).

[8] This is the procedure used by W. H. Morris-Jones and B. Das Gupta in "India's Political Areas: Interim Report on an Ecological Investigation," *Asian Survey,* 9 (June, 1969), 399–424; also by Donald S. Zagoria, "The Ecology of Peasant Communism in India," *Amer-*

The procedure also has important shortcomings, however.[10] Most serious is the likelihood of the ecological fallacy.[11] To aggregate a number of seats into one district (average population per district: more than 1½ million) and then treat the district as a unit involves the assumption that there is a common underlying behavior in the district as a whole; that is, that the various parts of the district are behaving electorally like the district. When the "parts" are constituencies for MLA's, the assumption is dubious, for each constituency is at least in some sense *sui generis*. In the larger districts, which may contain up to 30 MLA seats, the homogeneity assumption becomes virtually untenable. By using such data we can find out a great deal about district voting behavior, and this knowledge is in itself significant, but any inferences about constituency voting behavior are very tenuous indeed. The tenuousness is compounded by the fact that most of the non-Congress parties do not run candidates in every MLA seat, so that a party that runs just four candidates, all of whom win, might well have the same percentage of the district level vote (and the same statistical value) as a party that runs 15 losing candidates.

It is, of course, possible to aggregate to levels above the district. Elkins has found six regions in South India that differ significantly among themselves, while state-level ecological analyses have recently been published by Heginbotham and Weiner.[12] The state is a natural unit in the political sense, and is logically better suited for comparison with equivalent units than is the district. Also it is considerably easier to obtain current socioeconomic data at the state level.[13] But the states are scarcely electorally homogeneous within themselves; they vary hugely in size, and they are so few as to make statistical treatment extremely precarious.

It is also possible to aggregate data at levels below the district. One method is to use the constituency rather than the census/administrative area as the basic unit of analysis, as Benjamin and Blue have done.[14] Using seats for Members of the national Parliament (MP's) as their units, they have employed a method of fitting 330 districts to 480 constituencies in such a way that they end up with ten deciles of constituencies. This technique permits the use of the full range of socioeconomic data available at district level, while avoiding the previously mentioned problem of mixing several constituencies into a single unit. The disadvantage is that the resulting data must then be treated as deciles rather than as individual constituencies. One is left in effect with a sample size of ten, and must use rank-order comparisons like the Wilcoxon-Mann-Whitney

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ican Political Science Review, 65 (March, 1971), 144–160; and David J. Elkins, "Social Mobilization, Social Structure, and Politics: Evidence and Qualifications," a paper presented at the annual meeting of the American Political Science Association, Chicago, 1971. An earlier exercise along these lines is reported on in Government of India, *Census of India 1961*, Vol. I, *India*, Part I-A(i) and (ii), *Levels of Regional Development in India*, by A. Mitra (Delhi: Manager of Publications, 1965 and 1966). Much of the Chandidas project was devoted to fitting seats for members of the national Parliament to district areas, a much more difficult task, because these parliamentary boundaries do overlap district lines; see his "Ecological Correlates."

[9] See, for instance, the lists of predictor variables employed in Morris-Jones and Das Gupta, "India's Political Areas," and Zagoria. The large number of variables available at district level also permits the use of statistical techniques like principal components analysis to ameliorate the problems of multicollinearity among the predictors. See Biplab Das Gupta, "Socio-Economic Classification of Districts: A Statistical Approach," *Economic and Political Weekly*, 6 (August 14, 1971), 1763–1774; and also W. H. Morris-Jones and B. K. Das Gupta, "More Dealings with Districts," a paper presented at the annual meeting of the Association of Asian Studies, New York City, March 1972.

[10] Many of these shortcomings are pointed out by Morris-Jones and Das Gupta in "India's Political Areas."

[11] Since W. S. Robinson's classic essay in 1950, the literature on the "ecological fallacy" has become fairly extensive. Hayward R. Alker, Jr. surveys this literature in his "A Typology of Ecological Fallacies," in Dogan and Rokkan, pp. 69–86. For Robinson's original piece, see "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, 15 (August, 1950), 351–357.

[12] Elkins; Stanley J. Heginbotham, "The 1971 Revolution in Indian Voting Behavior," *Asian Survey*, 11 (December, 1971), 1133–1152; Myron Weiner, "Political Development in the Indian States," in *State Politics in India*, ed. Myron Weiner, (Princeton: Princeton University Press, 1968), pp. 3–58.

[13] For instance, from the yearbook put out by the Publications Division of the Ministry of Information and Broadcasting, Government of India *India: A Reference Annual*.

[14] Roger W. Benjamin and Richard N. Blue, with Stephen Coleman, "Modernization and Political Change: A Comparative Aggregate Data Analysis of Indian Political Behavior," *Midwest Journal of Political Science*, 15 (May, 1971) 219–261.

test or graphic analyses,[15] eschewing more precise statistical techniques such as multiple regression that are possible with larger populations and more exact measurements of each unit of observation.

If one is willing to forsake the rich lodes of district level data for the sparser socioeconomic deposits at subordinate levels, a good deal of worthwhile analysis is still possible. Brass has used as his unit of measurement the subdivision, an administrative (and census) area intermediate between the district and the MLA constituency.[16] There is an initial problem of some severity here, as MLA seats often cross the subdivisional boundaries, but Brass overcomes it by aggregating subdivisions up to the point where constituency and administrative boundaries do coincide. The resulting population of units is a good deal larger than would be the case if districts were used, and thus the individual unit is much closer to the MLA constituency level. The disadvantages are that the units vary considerably in size (depending on how many subdivisions are clustered together to make up any given unit), and as before, a number of MLA constituencies are being consolidated as one unit.

Matching Census and Electoral Geography

For some types of socioeconomic data, it is possible to overcome virtually all of the problems mentioned in the preceding section and to obtain a good fit between census and electoral units. The method used here is based on isoplethic mapping, which connects areas of equal concentration of population. Such an approach permits the researcher to use both the census data and all the voting data by individual MLA constituency.

Before getting into the details of isoplethic analysis, it would be useful to have a picture of Muslim distribution in the state as a whole. Bihar state had a Muslim minority of 13.48 per cent at the time of the 1971 census, as against an all-India figure of 11.21 per cent. This statistic represented a substantial growth for the Muslim community in Bihar, for it had increased its numbers by 31.3 per cent over the ten years since the 1961 census, while the state population as a whole grew by 21.3 per cent.[17]

The Muslims of Bihar are definitely not distributed evenly over the state. The seventeen districts of Bihar show considerable variation around the mean of 13.5 per cent, ranging from 4.0 per cent for largely tribal Singhbhum District in the southern Chotanagpur plateau to 39.9 per cent for Purnea District, which almost borders the new nation of Bangladesh in the east.[18]

As analysis proceeds to lower levels, the unevenness of the Muslim population becomes much more pronounced. For the 1961 census, communal data were published for the development block, or *anchal*, of which there are almost 600 in Bihar, averaging around 80,000 in population at the time of the census. Unlike many Indian socioeconomic variables, the Muslim distribution among these anchals is geographically continuous and so lends itself to the isoplethic mapping technique. Figure 1 offers an isoplethic map of Bihar's Muslim population based on data for the individual anchals.[19] Points of equal Muslim "density" are connected by lines, similar to the elevation lines on a topographic map or the isobars on a weather map.

The most striking aspect of the map is the extremely heavy concentration of Muslims in the most easterly part of Purnea District, where a number of blocks are more than 50 per cent

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[15] *Ibid.* This method is also limited to Parliamentary seats, as the state Legislative Assembly seats are far too small (usually 1/6 of a Parliamentary seat) to be compared individually with districts.

[16] Paul R. Brass, "Ethnic Cleavages and the Punjab Party System," a paper presented at the annual meeting of the Association of Asian Studies, New York City, March 1972. Yet another method is to select a sample of Legislative Assembly seats that do fit the census boundaries exactly; the difficulty here is that most of the seats do not fit and so must be thrown out of the analysis. See Harry W. Blair, "Elections and Aggregate Data Analysis at the Block Level in Bihar, 1967–1969," also a paper presented at the March 1972 meeting just noted.

[17] Government of India, *Census of India 1971*, Series I—India, *Paper 2 of 1972: Religion*, by A. Chandra Sekhar (Delhi: Manager of Publications, 1972), pp. 16, 95. Bihar's Muslim population grew by 13.9 per cent during 1931–41, decreased by 7.3 per cent during 1941–51, then increased by 32.3 per cent during 1951–61. Apparently what had occurred during the 1950s was a significant reverse migration from East Pakistan back into Bihar after the large outward flow during and after the Partition of 1947. See Government of India, *Census of India 1961*, Vol. IV, *Bihar*, Part I-A(i), *General Report on the Census*, by S. D. Prasad (Patna: Bihar Secretariat Press, 1968), pp. 484–485. The Hindu-Muslim ratio of about 7-to-1 in 1961 and 1971 fits roughly into what Mason would call a "competitive" type; cf. *Patterns of Dominance*, pp. 60–65.

[18] Purnea did in fact border East Pakistan after the Partition, but a slice of about 700 square miles was transferred to West Bengal in 1956, as a part of the States Reorganization Act. See Marcus F. Franda, *West Bengal and the Federalizing Process in India* (Princeton: Princeton University Press, 1968), pp. 8–61.

[19] Data from which the map was derived were obtained from the district census handbooks that formed part of the 1961 census, e.g., Superintendent of Census Operations, Bihar, *Census 1961*, *Bihar District Census Handbook 1: Patna*, by S. D. Prasad (Patna: Secretariat Press, 1966), pp. 90–93. Data for the 1971 census by block had not become available by the time of writing this article.

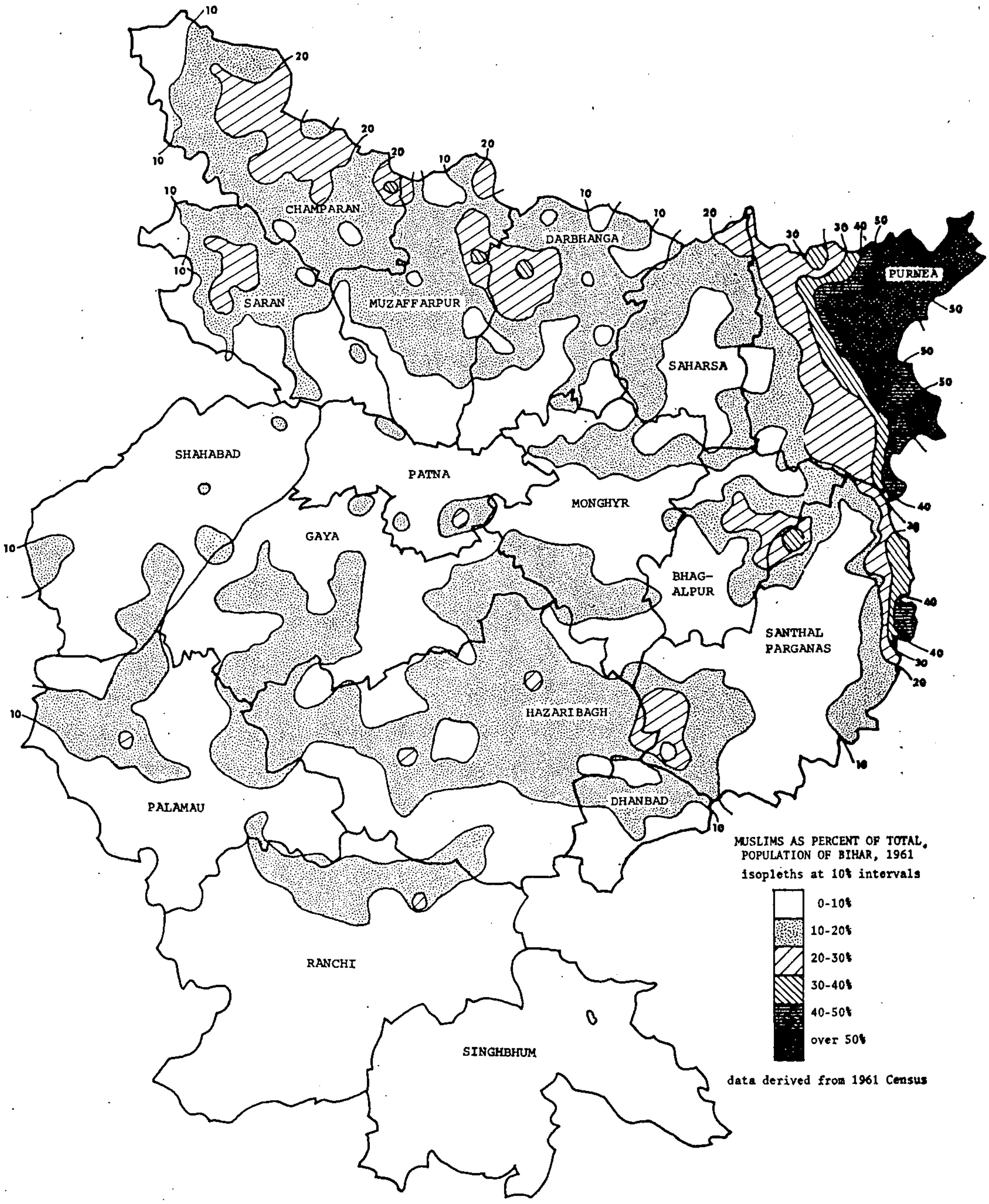
CHAMPARAN
SARAN
MUZAFFARPUR
DARBHANGA
SAHARSA
PURNEA
SHAHABAD
PATNA
MONGHYR
GAYA
BHAG-
ALPUR
SANTHAL
PARGANAS
HAZARIBAGH
PALAMAU
DHANBAD
RANCHI
SINGHBHUM
10
20
30
40
50
MUSLIMS AS PERCENT OF TOTAL,
POPULATION OF BIHAR, 1961
isopleths at 10% intervals
0-10%
10-20%
20-30%
30-40%
40-50%
over 50%
data derived from 1961 Census

Figure 1. Muslim Population in the Districts of Bihar.

Muslim. Though they are less immediately apparent, there are other considerable concentrations as well. The northern districts of Champaran, Muzaffarpur, Darbhanga, and Saran all have sizable concentrations that are more than 20 per cent Muslim, and the first three have small pockets that are more than 30 per cent. Perhaps the most interesting distribution is that found in Santhal Parganas, which has one area that is more than 40 per cent Muslim, and two others that exceed 30 per cent, while at the same time more than half the district is less than 10 per cent Muslim. Also of interest is the broad swath of territory running through the north central part of the state from west to east (roughly along the path of the Ganges River) in which the Muslim concentration is very low, in fact less than one per cent in many blocks. The southern districts of Chotanagpur have few Muslims, except for a few small areas with concentrations of more than 20 per cent.

Muslims in the Electoral Process

When examined at the statewide level, the data on voting for Muslim candidates, presented in Table 1, show no general secular trend over the six elections that have been held since 1952.[20] As might be expected from the Western experience, the minority Muslim community has in every election at both state and national level polled substantially fewer votes for its candidates than its 12.5 per cent of the population. Likewise, the number of Muslims elected to office has been far fewer than would be the case if they were proportionately represented.[21]

The vote given to Legislative Assembly candidates rose during the first three general elections,[22] and then peaked out at just under eight per cent in the 1962 poll, after which it

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[20] The first four general elections included both the state Legislative Assembly and the national Parliament. Because of the chronic instability that characterized state politics after the 1967 elections, the Assembly was dissolved in 1968 and new elections were held in 1969. Elections for the Parliament, due by the five-year constitutional requirement to be held at least by 1972, were called a year early by Mrs. Gandhi and thus were held in 1971. The Assembly held its sixth election in March 1972.

[21] The United States House of Representatives elected in 1972, for example, had only 11 black members out of the total 435, while the population of the country is about 11 per cent Negro. For an analysis of Muslim representation in another state of India (Andhra Pradesh), see Rasheeduddin Khan, "Muslim Leadership and Electoral Politics in Hyderabad: A Pattern of Minority Articulation," *Economic and Political Weekly* 6 (April 10, 1971), 783–794 and (April 17, 1971), 833–840, esp. 837.

[22] A study based on the first three elections concluded, partly on the basis of data similar to those given here for the 1952–1962 period (i.e., showing a rising proportion of the vote for Muslim candidates), that the Muslim community was being successfully integrated into the Indian political system. See Gopal Krishna, "Electoral Participation and Political Integration," *Economic and Political Weekly,* 2 (Annual Number, February 1967), 179–190. See also, in rebuttal, Imtiaz Ahmed, "Indian Muslims and Electoral Politics," *Economic and Political Weekly,* 2 (March 11, 1967), 521–523; and more recently Ahmed's "Secularization," *Seminar,* 144 (August 1971), 22–26; and in addition, Rajni Kothari *Politics in India* (Boston: Little, Brown, 1970), pp. 245–247.

Table 1. Muslim Candidates in Bihar: Vote Polled, Candidates and Winners, 1952–1972

Legislative Assembly (MLA's)	1952[c]	1957	1962	1967	1969		1972
% of valid vote polled by Muslims	4.53	5.84	7.88	6.80	6.27		7.45
Muslims as % of candidates	4.70	6.73	7.19	8.00	6.18		8.17
(*n*)[a]	(72)	(90)	(110)	(162)	(133)		(162)
Muslims as % of winners	7.01	7.86	6.60	5.66	5.97		7.86
(*n*)[b]	(22)[d]	(25)	(21)	(18)	(19)		(25)
Parliament (MP's)	1952[e]	1957	1962	1967		1971	
% of valid vote polled by Muslims	2.35	4.39	6.31	4.20		3.37	
Muslims as % of candidates	2.03	5.29	8.15	5.08		6.41	
(*n*)[a]	(4)	(10)	(19)	(16)		(27)	
Muslims as % of winners	5.66	5.66	3.77	3.77		5.66	
(*n*)[b]	(3)	(3)	(2)	(2)		(3)	

[a] Number of Muslim candidates.

[b] Number of Muslims elected.

[c] In the 1952 Legislative Assembly election, there were a total of 330 seats in Bihar. The major part of the area composing 14 of these seats (4 in Purnea District and 10 in the former Manbhum District) was transferred to West Bengal in 1956, and data on these seats (2 of them won by Muslims) are not included here. Since 1957 there have been 318 seats.

[d] Includes one Muslim Congress MLA returned in an uncontested election.

[e] In the 1952 Parliamentary election, there were 55 seats allotted to Bihar. The major part of one double-member constituency (i.e., 2 seats) in Manbhum District was transferred to West Bengal in 1956, and data on it are excluded here. Since 1957 there have been 53 seats.

Source: Official returns through 1969, newspaper reports for 1971 and 1972 elections.

declined to a bit over six per cent in 1969, then rose again in 1972. The national Parliament vote for Muslims has lagged behind the Assembly vote by at least 1½–2 per cent in each election, with the gap increasing to more than 4 per cent if the 1971 and 1972 contests are compared. The disparity may be due to the difference in scale of constituency. Six seats for members of the Assembly are contained within each seat for a member of the Parliament, so that while the MLA candidates may be able to capitalize on small pockets of Muslim concentration, the MP seats cover such a wide area that these pockets are of negligible importance. Thus more Muslims are willing to contest at the Assembly level and, whether as cause or effect of this tendency, Muslim candidates collectively do better at the lower level.

The number of Muslim candidates particularly at the MLA but also at the MP level has gone up as the overall number of entrants has increased.[23] In 1952, there were only four Muslim MP candidates, about two per cent of the total, the proportion rose a bit over each succeeding election through 1962, then leveled off at around five or six per cent.

The number of candidates, though, may not be a very accurate measure of minority political strength, for anyone who wants to put up the required deposit of 250 rupees may declare himself an MLA candidate in an election, and for twice that sum he may file nomination papers as an MP contestant. This is not a great deal of money, and consequently there are large (and apparently increasing) numbers of publicity seekers and "nuisance candidates" who are sponsored by more powerful political operators to drain off a few votes from an opponent, but who have not the remotest chance to win.[24] In the 1967 Assembly election, 49.3 per cent of the entrants each polled less than 10 per cent of the vote, in 1969 the portion rose to 56.7 per cent, and in 1972 dropped very slightly to 54.0 per cent.[25] Muslim candidates reflected this general pattern, with 53.1 per cent of the MLA contestants each receiving under 10 per cent of the vote in 1967, 54.1 per cent in 1969, and 56.8 per cent in 1972.

Considerably more important than the number of candidates in the race, of course, is the number that win. Here the Muslims evidence roughly the same patterns as in the two categories already considered. In the Assembly, the number of MLA's elected began at 22, or about 7 per cent, increased to almost 8 per cent, dipped to a little less than 6 per cent in 1967 and 1969, then recovered to the 1957 level in the last election. The MP's returned from the Muslim community have held steady at 2 to 3 per cent throughout the period.

In terms of proportional equity, Muslims have had for most of the period about half as many representatives at both levels as they would be entitled to on the basis of strict proportionality. It might be pointed out, though, that they are by no means the most slighted group in Bihar in the way of underrepresentation. As has been shown elsewhere, lower caste Hindu communities such as the Kahars, Nais, Dhanuks, Tatwas, and Telis, all of whom have significant populations in Bihar, have been represented in the Vidhan Sabha much more poorly than the Muslims, while upper caste groups of the "twice-born" strata, like Brahmans, Bhumihars and Rajputs, have been and are grossly overrepresented.[26]

The Geography of Muslim Voting

The geographical pattern among the MLA seats won by Muslims emerges in Figure 2. In the 1950s, when the Congress was at the zenith of its power, both in national and state politics, it regularly won with overwhelming majorities at the polls. In 1952 and 1957 the Congress swept more than two-thirds of Bihar's MLA contests, including in both cases all but one of the Constituencies carried by Muslim candidates. The 1952 and 1957 maps in Figure 2 indicate some victories in the heavily Muslim area of eastern Purnea District (cf. Figure 1 for district names and Muslim population), also in those regions of Champaran, Saran, Darbhanga and Bhagalpur districts where Muslims amounted to more than 20 per cent of the population.[27] There were almost as many seats

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[23] From 1532 to 1983 for MLA's during 1952–1972 (the maximum was 2153 in 1969), and from 197 to 421 for MP's during 1952–1971.

[24] On nuisance candidates in Bihar, see Harry W. Blair, "Caste, Politics and Democracy in Bihar State, India: The Elections of 1967" (Ph.D. thesis, Duke University, 1969), pp. 146–148.

[25] To cut down on the number of petty candidates, the government requires that the nomination deposit be forfeited if a candidate does not get at least one sixth (16⅔%) of the vote. This discouragement seems ineffective, however, for 1301 MLA entrants lost their deposits in 1967, 1366 in 1969, and 1254 in 1972.

[26] Harry W. Blair, "Ethnicity and Democratic Politics in India: Caste as a Differential Mobilizer in Bihar," *Comparative Politics*, 5 (October, 1972), 107–127. This pattern of inequality has continued through the 1972 election as well; see N. K. Singh, "Bihar: Many Faces of Caste Politics," *Economic and Political Weekly*, 7 (April 8, 1972), 748–749.

[27] The 1961 population data are being projected backward in time, a practice which necessarily entails the possibility of inaccuracies, just as does the forward projection to 1972 later on in this paper. In effect, it is being assumed that the Muslim population of Bihar is expanding at roughly the same rate as the population as a whole. This was distinctly not the case in the 1940s

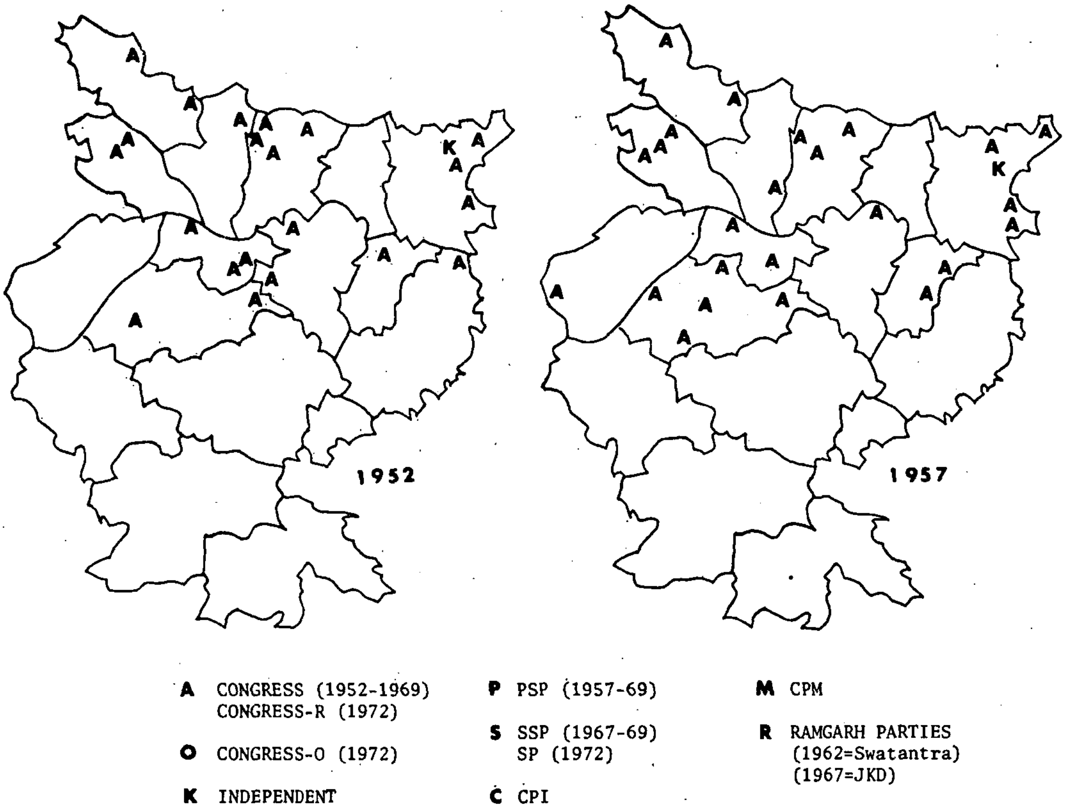
1952
1957
A CONGRESS (1952-1969) CONGRESS-R (1972)
O CONGRESS-O (1972)
K INDEPENDENT
P PSP (1957-69)
S SSP (1967-69) SP (1972)
C CPI
M CPM
R RAMGARH PARTIES (1962=Swatantra) (1967=JKD)

Figure 2. Muslims Elected to Bihar Legislative Assembly, 1952–1972.

Note: Boundaries for 1952 map have been adjusted to compensate for changes made after that election (cf. note c to Table 1). Between the 1962 and 1967 elections, a small change was made in the boundary dividing Monghyr and Saharsa Districts (cf. Figure 1); this change is reflected in the maps here.

won, however, in areas of very negligible Muslim concentration, such as the seats in Shahabad, Patna, Gaya, and Monghyr districts. Altogether 21 of the 47 seats won by Muslims in the two elections were located in areas where there were fewer than 20 per cent Muslims.

One could point to the halcyon days of communal toleration during the 1950s as the reason behind this success, and indeed that decade does seem in retrospect a calm interlude between the violence of the Partition and the communal riots that were to bedevil Bihar in 1964 and 1967. A more likely explanation lies in the Congress hegemony of the time; it could and did elect almost anybody it chose to run anywhere.[28]

In 1962, Congress strength diminished somewhat in the state as a whole, and we find corroborating evidence in the map, for this time only 15 of the 21 Muslim MLA's were returned by the Congress. The new Swatantra Party under the leadership of the Raja of Ramgarh won 50 seats in Bihar (giving it second rank in the state assembly), including four filled by Muslims. All told, nine seats were won

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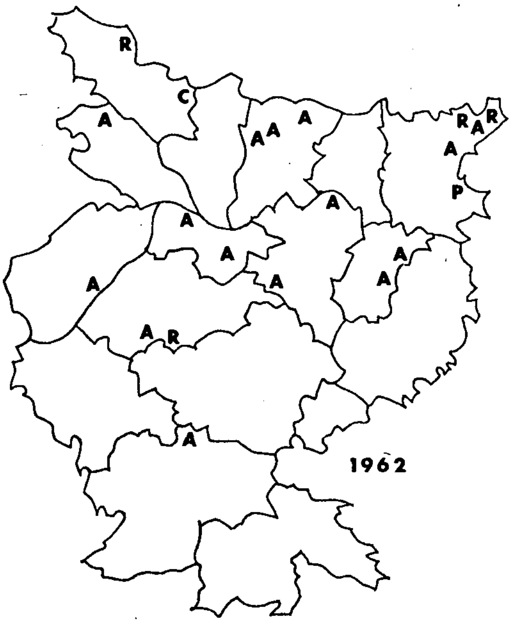
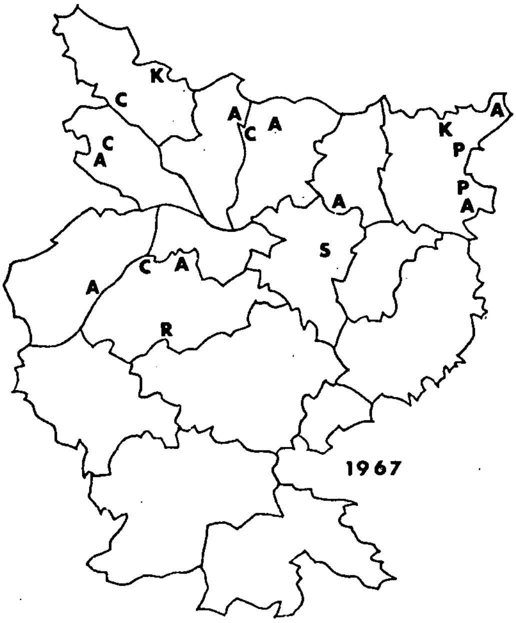
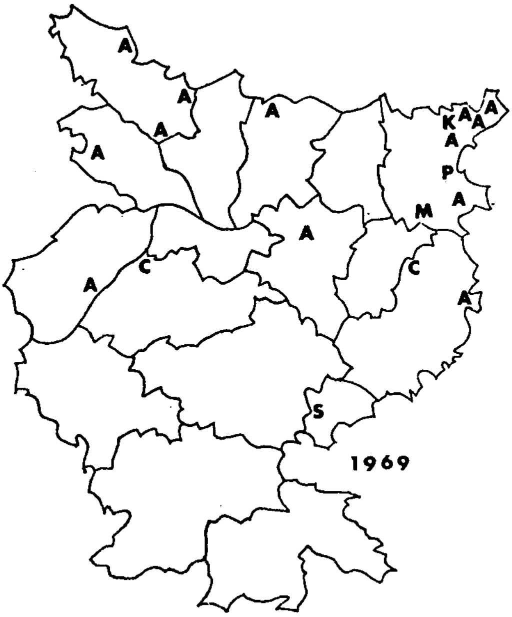
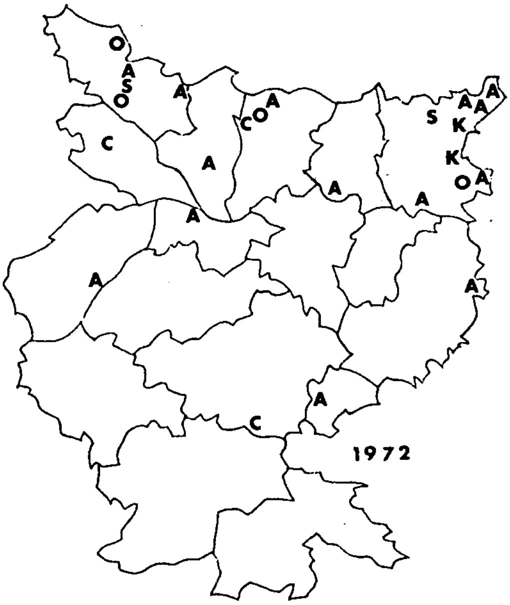
and early 1950s (cf. note 17, above) but is, one hopes, not an unreasonable assumption for the period since then. A caveat should also be added concerning constituency boundary changes over time. Because the geographical delimitation of seats was changed for the 1957 elections and then again for the 1967 elections, statistically precise comparisons cannot be made across all six elections for all seats, though a great many seats have remained almost intact geographically ever since 1952.

[28] It might be pointed out that while the Congress was electing so many Muslim MLA's in the 1950s, Muslim representation on the vitally important Bihar Pradesh Congress Executive Committee was significantly diminishing. In the 1930s and 1940s, Muslim membership on this committee averaged about 20 per cent, but in the 1950s it decreased to the level of 5–6 per cent. See Ramashray Roy, "Dynamics of One-Party Dominance in an Indian State," *Asian Survey,* 8 (July, 1968), 553–575; and his "Caste and Political Recruitment in Bihar," *Caste in Indian Politics,* ed. Rajni Kothari (New Delhi: Orient Longmans, 1970), pp. 228–258 at 242–244.









by Muslims in areas of low (under 20 per cent) Muslim dominance.

The fourth general elections of 1967 saw the eclipse of Congress rule in Bihar, and the Congress share of the Muslim MLA's dropped as well, to only 8 of 18. The Communist Party of India (CPI) picked up four, the Praja Socialist Party (PSP) two, and its rival, the Samyukta Socialist Party (SSP) took one. The Ramgarh group had collapsed the Swatantra Party by moving out, detouring through a dalliance with the Congress in 1966 and into a new party called the Jana Kranti Dal (JKD), which returned one Muslim MLA.

In the midterm poll of 1969, the Congress again failed to capture a majority of seats in the Legislative Assembly, but it did improve its Muslim holdings from 8 to 12 seats; the CPI picked up two; while its rival, the Communist Party of India (Marxist)—usually abbreviated CPM—took one: and the Praja and Samyukta Socialists held at one each. More significantly, a trend that had begun in the 1962 election had now become quite apparent. There were now only 6 Muslim MLA's from areas that had less than 20 per cent Muslim population, while 13 were returned from regions that were more than 20 per cent Muslim, and 8 of these held seats from areas where the minority comprised more than 40 per cent.

Between the 1969 and 1972 elections came the split in the Congress that resulted in a majority group, Prime Minister Indira Gandhi's Congress (Ruling), or Congress-R, and a smaller dissident group, the Congress (Opposition), or Congress-O. In Bihar, as in most of the states, the Congress-R claimed the major share of the former united Congress MLA's. In the 1972 election the Prime Minister's Congress improved its position greatly by winning a majority of 167 seats in the Assembly, an outcome reversing that of the 1967 and 1969 polls.

The return to Congress hegemony also saw a return in Muslim representation to the levels of the 1950s. Altogether 25 Muslims became MLA's, the same number as in 1957 (see Table 1). Some 14 Muslims won MLA seats on the Congress-R ticket, against only 4 for the Congress-O, and 7 for other parties and Independents. The 1972 geographical pattern shows some degree of resemblance to the 1957 distribution but more similarity to that of 1969: a concentration of MLA's in the heavily Muslim areas of Purnea and Champaran districts, with fewer scattered elsewhere. As in 1969, 8 of the 25 were elected from areas more than 40 per cent Muslim, against 5 in 1957, while only 8 of the 1972 contingent came from areas of less than 20 per cent Muslim population, as opposed to 12 in the 1957 contest.

From the maps in Figure 2, it would appear that across the six elections Muslims are becoming not more but less integrated within the political system. It is becoming easier for Muslims to get elected from areas where their coreligionists are strong in numbers, but at the same time it is becoming more difficult (though there are significant exceptions) for members of the minority to win office where their numbers are weak. This conclusion will be strengthened in the next section.

Statistics of Muslim Population and Voting Patterns

We now move to a consideration of the statistical relationships between the measures of Muslim population developed from Figure 1 and voting patterns at the constituency level. Upon a map similar to that used in Figure 1 (but larger in scale, so as to permit a greater degree of exactness) a second map, containing an outline of the MLA constituencies for the 1957 and 1962 elections was superimposed. An estimate of the Muslim population for each seat could then be gained by observing the pattern of isopleths within its area, and each seat was then given a code for one of six ordinal categories.[29] The process was repeated for the 1967 delimitation, which was in effect for the 1969 and 1972 elections as well,[30] thus making it possible to compare a measure of Muslim population directly with election results for individual Legislative Assembly constituencies.

Correlations between Muslim population and vote for Muslim candidates over the last five elections appear in Table 2.

Here we find confirmation of the data previously explored in Figure 2, though the shift is not evident until the end. There is a barely perceptible rise in the correlation for 1962 over that for 1957, meaning that vote for Muslim

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[29] The ordinal categories used were 0–5 per cent, 5–10 per cent, deciles up to 40 per cent over 40 per cent. A test for accuracy was made by comparing the estimates arrived at in this fashion with direct census data on Muslim population for 69 MLA seats scattered around the state that actually do match up with the census units. The correlation (r) between the Muslim population estimated from the isoplethic map and the actual Muslim population for the 69 seats was .939. Using r^2, we could say that the degree of "congruence" was 89 per cent.

[30] Because of difficulties in the delimitation for the 1952 election, this analysis was not performed for the first general elections. No attempt was made to perform the same technique for Parliamentary constituencies, which are each six times the size of a State Assembly seat, because it was thought that the problem of "ecological fallacy" was too great. A number of techniques have been developed by geographers for working out correspondences between maps using different areal units. See, for instance, Arthur H. Robinson, "Mapping the Correspondence of Isarithmic Maps," *Annals of the Association of American Geographers,* 52 (December, 1962), 414–425. (The terms "isarithmic" and "isoplethic" are often used interchangeably.) There has, however, been little interest in using such techniques on electoral data. For an exception, see Peirce F. Lewis, "Impact of Negro Migration on the Electoral Geography of Flint, Michigan, 1932–1962: A Cartographic Analysis," *Annals of the Association of American Geographers,* 55 (March, 1965), 1–25.

Table 2. Correlations Between Muslim Population and Vote for Muslim Candidates for Bihar Legislative Assembly, 1957–1972[a]

	1957	1962	1967	1969	1972
Pearson *r*	.568	.574	.545	.684	.678
Number of seats[b]	59	84	100	95	96

[a] All correlations are significant at the .001 level.

[b] This figure represents the number of MLA seats which Muslim candidates contested. In 1957 there were Muslim entrants in ten additional seats, which are excluded here because they were double-member constituencies and could not be compared on the same basis as the single-member seats. Double-member seats were eliminated after 1957.

candidates was slightly more closely related to Muslim percentage of population in the second election.[31]

In 1967 there was a very slight drop and then in 1969 a considerable jump to .684, yielding an r^2 which accounts for about 47 per cent of the variance, as against the 32 per cent provided by the 1957 data. The 1972 data show a persistence of the 1969 pattern. There does indeed seem to have been a shift in Muslim vote-getting power to the Muslim areas of the state.

This change in voting patterns for Muslim candidates has not been paralleled by any sweeping shifts in the relationship of Muslim population to the electoral fortunes of a particular party, as is seen in Table 3.

In 1952, 21 of the 22 Muslim MLA's were Congressmen, and in 1957 the number rose to 24 of 25. Muslims were generally viewed as a

Table 3. Correlations Between Legislative Assembly Vote by Party and Muslim Population in Bihar, 1957–1972

	1957[a]	1962	1967	1969	1972
Congress[b]	.233** (210)[c]	.139* (318)	−.007 (318)	.276** (318)	.093 (261)
Congress-O					−.072 (269)
CPI	−.401** (40)	−.076 (85)	−.064 (97)	−.101 (165)	−.055 (55)
Socialist/SSP[d]		−.116 (131)	−.135 (200)	−.037 (193)	.022 (252)
PSP	.076 (150)	.166* (199)	.332** (181)	.132 (94)	
Jana Sangh	−.250 (21)	.045 (75)	−.052 (268)	.153** (301)	−.005 (265)
Ramgarh Parties[e]	.001 (78)	.053 (259)	−.055 (164)	.012 (137)	
Shoshit Dal				−.103 (125)	

* Indicates correlation significant at .025 level.

** Indicates correlation significant at .001 level.

[a] Includes only single-member seats for 1957. Subsequent elections eliminated the double-member constituencies.

[b] Indian National Congress through 1969, Congress-R in 1972.

[c] Number in parentheses indicates candidates fielded by that party in that year (number on which correlation is based).

[d] Socialist Party in 1957 and again in 1972, SSP in 1967 and 1969.

[e] Janta Party in 1957 and 1969, Swatantra in 1962, JKD in 1967.

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[31] It cannot, of course, be inferred from the increase in correlation that a greater proportion of Muslims voted for Muslim candidates in the 1962 election than in 1957, though that is probably the case. Given the statistics of Table 2, it is possible (albeit unlikely) that Muslims as individuals turned away from Muslim candidates in 1962. All we can say definitely is that Muslim candidates received more votes in general in areas more heavily populated with Muslims.

special constituency of the Congress, under the protection and patronage of Maulana Azad and others at the national level. The correlation of .233 for the 1957 election is evidence of this special relationship, in that the Congress did do significantly better in the Muslim areas than the other parties did.[32] The correlation dropped considerably in the 1962 election, however, and even more five years later.

There were numerous rumors in Bihar at the time of the 1967 election that Muslims were deserting their traditional support of the Congress.[33] At least some of these rumors had a foundation in the activities of the Majlis-e-Mushawarat, a politically oriented Muslim association formed in 1964. Although the Majlis backed Congress candidates for about half the seats in the Bihar Assembly, it openly supported non-Congress contestants for the other half.[34] The Majlis' effect on Muslim voters cannot be discerned from the present data, but it is the case that Congress support in Muslim areas dropped to the extent that the correlation between Muslim population and Congress electoral support completely vanished in 1967.

In 1969 the trend reversed itself. Though the Congress won ten fewer seats in the state as a whole and polled almost three percentage points less in the popular vote than in 1967, it did quite well in the Muslim areas. The r-value of .276 corresponds well with the impression given by the 1969 map in Figure 2, which showed Congress winning most of the Muslim seats in northeastern Bihar.

Whatever relationship was restored in the 1969 election disappeared in 1972, perhaps as part of the fallout from the Congress split. The Congress-R managed to retain a slight correlation with Muslim population, though not a statistically significant one, while the Congress-O wound up with a negative relation, even though it ran 30 Muslim candidates, as against only 28 for the Congress-R.

Both the Communist Party and Samyukta Socialist (in 1972 simply the Socialist) Party claim in their propaganda to be especially interested in the welfare of the Muslim minority, but support for both parties has been negatively correlated with Muslim population in all five elections, except in the case of the Socialists in 1972, when the correlation was a scarcely noteworthy correlation of +.022. The Shoshit Dal (literally, "exploited movement") arose as a short-lived splinter group from the SSP in late 1967, announcing that it, unlike its parent party, would devote itself to the betterment of the "backward" communities of Bihar, including Muslims.[35] In electoral behavior, however, it followed the ancestral pattern in finding its votes negatively correlated with Muslim population in its only time at the polls in 1969.

The Communists, on the other hand, were thought by some to be getting support from unexpected Muslim quarters in 1969, for there were a number of rumors to the effect that the Muslims of Bihar had received directions from the "ruling group" in Pakistan to vote for the CPI.[36] The correlation of −.101 between Muslim population and CPI vote in Table 3 gives little support to this belief.

Alone among the leftist parties, the PSP's electoral support has a positive correlation with Muslim population, reaching a high-water mark of .332 in 1967, when two PSP Muslims were returned as MLA's. Between 1969 and 1972 the PSP and SSP merged to form the new Socialist Party (SP), and it may very well have been the legacy of PSP popularity in Muslim areas that accounted for the very slight positive correlation between Socialist vote and Muslim population in 1972.

On the right, the various parties led into the electoral fray by the late Raja of Ramgarh have never displayed any degree of voting strength related to Muslim density, though the Raja's 1962 political vehicle, the Swatantra Party, did elect four Muslim MLA's. The primary reason for the negligible correlations probably lay in the source of the Raja's electoral strength, his home base and vast landholdings in Hazaribagh District, where he invariably won most of the seats (in many cases with his own relatives) and where the Muslim population is relatively small.

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[32] Because of the constituency delimitation problem mentioned above, correlations were not calculated for the 1952 elections. Again, it must be emphasized that Table 3 does not measure the relationship between voting *by Muslims* and voting for particular parties. It only measures the relationship between *Muslim population* and voting for the individual parties.

[33] As reported to the author, who was in Bihar at the time of the 1967 elections.

[34] Zaheer Masood Quraishi, "Electoral Strategy of a Minority Pressure Group: The Muslim Majlis-e-Mushawarat," *Asian Survey,* 8 (December, 1968), 976–987. Whatever success the Majlis had was rather short-lived, for it had more or less collapsed by the time of the 1969 poll. See the same author's "Emergence and Eclipse of Muslim Majlis-e-Mushawarat," *Economic and Political Weekly,* 6 (June 19,1971), 1229–1234; also Theodore P. Wright, Jr., "Muslims as Candidates and Voters in 1967 General Elections," in *Fourth General Elections in India,* ed. S. P. Varma and Iqbal Narain, vol. 2 (Bombay: Orient Longmans, 1970), pp. 207–224.

[35] On the Shoshit Dal, see Blair, "Caste, Politics and Democracy," pp. 334–338.

[36] Vishwanath Prasad Varma, "Analysis of Results," in *A Study of Mid-Term Elections in Bihar (1969),* ed. V. P. Varma (Patna: Institute of Public Administration, Patna University, 1970), pp. 76–77; also his "District-wise Classification of Results," in the same volume, pp. 94–95 and 101.

It has almost become part of the conventional political wisdom in India that the Hindu-oriented Jana Sangh does very well in Muslim areas, because the more Muslims that are visible, the more easily the Jana Sangh can employ its ability to arouse communal feeling among the Hindus and translate this animosity into votes for itself. This notion finds superficial support in the observation that in both 1967 and 1969, urban MLA seats in eastern Patna, Gaya, Bhagalpur and Ranchi towns, all of which have some Muslim concentration, were carried by the Jana Sangh.[37] But when we look at the state as a whole, the relationship tends to disappear.[38] Through 1967 the correlation is weak, and even negative in two out of three cases. In 1969 there is a significant though low correlation, but in 1972 the pattern returns to that displayed in previous elections.

Conclusion

In terms of representation in the Legislative Assembly, Muslim fortunes have oscillated, up to a high of 25 MLA's in 1957, down to 18 or 19 in the late 1960s and then back up to 25 in the 1972 poll. The portion of the vote obtained by Muslim candidates has undergone a similar oscillation. At the same time, however, there has been a more definite one-way trend in the geographical distribution of vote for Muslim candidates, who have been getting their votes more and more from areas of substantial Muslim population. In statistical terms, the Muslim population "explained" a good deal more of the vote for Muslim candidates at the end of the period than at the beginning. Muslim numbers have been making themselves felt politically in the last two elections, but what appears to be increasing Muslim cohesiveness in these areas is coming at the cost of Muslim candidates' ability to exert appeal in regions of little Muslim numerical strength. In terms of the dilemma posed at the beginning of this paper, solidarity appears to be winning over integration.[39]

Still, there is considerable evidence that integrative factors have not completely disappeared. For one thing, not all the Muslim MLA's get elected from Muslim strongholds. Some Muslim politicians do continue to poll high percentages of the vote in areas where their co-religionists are few. Indeed, the fact that in this paper we can refer to areas of 20 and 30 per cent Muslim population as "strongholds" indicates that any real communal polarization is a long way off. Equally important, the low correlations between Muslim population and vote for any particular party, especially in the most recent election, mean that no party is wedded in its electoral fortunes to the statistics of communal demography. Further, there is no evidence from past elections that any party would stand to gain greatly if at all by an appeal to communal feelings.

In all political systems the dilemma between integration and sociocultural identity that faces minority groups is at bottom an insoluble one, for generally both goals are deeply desired, but the complete realization of either one must necessarily mean the repudiation of the other. The most satisfactory arrangement would appear to be that employed by many ethnic groups in America: pursuit of a portion of both goals simultaneously, so that while neither is totally achieved, both are in part accomplished. If that is so, Bihar's Muslims are probably making the best of their opportunities; more Muslims are getting elected in Muslim areas, but Bihar politics has not become polarized along communal lines, for some Muslims can still be elected from non-Muslim areas.

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[37] There are only 13 seats out of the total 318 in Bihar that could be called in any way genuinely urban. The Jana Sangh won 2 of the 13 in 1972.

[38] Craig Baxter disputes the existence of such a relationship in *The Jana Sangh: A Biography of an Indian Political Party* (Philadelphia: University of Pennsylvania Press, 1969), pp. 234–235. Angela Burger also takes issue with the thesis in her Uttar Pradesh study, *Opposition in a Dominant Party System* (Berkeley: University of California Press, 1969), pp. 199–200. But see also Morris-Jones and Das Gupta, "India's Political Areas," 416; B. D. Graham, "A Report on Some Trends in Indian Elections: The Case of Uttar Pradesh," *Journal of Commonwealth Political Studies,* 5 (November, 1967) 179–199 at 191; and Imtiaz Ahmad, "Religion in Politics: A Comment," *Economic and Political Weekly,* 7 (January 8, 1972), 81–86 at 84–85.

[39] For other electoral systems, evidence for the salience of religious cleavage appears mixed; in some it continues to be of major importance, while in others it is barely noticeable in electoral behavior. See Arend Lijphart's study of 10 Western systems, "Class Voting and Religious Voting in the European Democracies," Survey Research Centre, Occasional Paper No. 8 (Glasgow: University of Strathclyde, 1971).

Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation*

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When *The New York Times* pointed out on June 5, 1969, that "voting apathy among blacks" was to blame for the defeat of all but one black candidate for Mayor in Mississippi's 1969 municipal elections, it was reflecting quite accurately the accepted political science wisdom about voter participation. In a rare show of unanimity, political scientists have generally taken as given that nonparticipation in politics is a result of disinterest or apathy, and have focused their research on specifying the exact social, economic, and attitudinal correlates of such apathy. And even in this search for correlates of apathy, there is substantial agreement, for virtually every study of political participation has concluded that participation is lowest (because apathy is highest) among the poor and the less well educated, whether in the United States or elsewhere. The only debate has been about why this should be so, with imaginative social scientists advancing explanations as diverse as "class differences in child rearing practices" and "differential political role expectations."[1]

For all its impressive empirical support, however, the "income-education-apathy thesis" may have a potentially fatal flaw: It assumes that people fail to participate in politics chiefly because they do not think it is worth the time or because they fail to understand what is at stake. It begins, in other words, with a conceptual framework attuned to political life in a modern democracy, where participation is truly open.

But how appropriate is this framework for understanding political participation patterns where participation involves more than mere time and understanding—that is, where it involves serious risks as well? In such situations, the real explanation of variations in political participation rates may not be apathy at all, but *fear*—fear of violence, or more subtle forms of fear related to social and economic coercion.

Significantly, however, most students of political participation have neglected the impact of fear on participation, and have downplayed the coercive aspects of political life. Robert Lane, for example, identifies no less than eleven factors that purport to account for lower-class nonparticipation in politics, but *fear* is not among them.[2] Lipset and Milbrath develop their own catalogues of causes, but with the same result: virtually no mention of fear.[3] Apathy, it seems, comes in 57 varieties, but fear in none.

The one recent empirical analysis of political participation that even treats the subject of fear is the massive Matthews and Prothro analysis of Negro politics in the American South. But even Matthews and Prothro manage to dispense with fear in a hasty eleven pages out of 488, concluding, on the basis of measures of fear that we seriously question for reasons elabo-

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[1] The literature on political participation is voluminous. Seymour Martin Lipset summarizes much of the empirical work linking nonparticipation to low income and education in "Who Votes and Who Doesn't?" in Lipset, *Political Man* (Garden City, New York: Anchor Books, 1963), pp. 183–229; and Robert Lane summarizes the theories purporting to explain this link in *Political Life* (Glencoe, Ill.: The Free Press, 1959), pp. 220–234. See also: Bernard Berelson, Paul F. Lazarsfeld and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954); Philip K. Hastings, "The Voter and the Non-Voter," *American Journal of Sociology*, 62 (November, 1956), 303–04; Lester Milbrath, *Political Participation* (Chicago: Rand-McNally Co., 1965).

[2] The closest Lane comes to acknowledging a role for fear is in his discussion of the low sense of political efficacy and limited self-confidence among the poor. Lane, *Political Life*, pp. 222–34.

[3] Lipset, "Who Votes?" in *Political Man;* and Milbrath, *Political Participation,* particularly pp. 48–141.

rated below, that "intimidation and fear" are "*not* major causes" of Negro failure to participate in politics as much as whites do.[4] This finding is all the more surprising because it flies in the face of virtually every recent participant-observer account of southern black politics. According to one such study, voter registration workers in the South during the 1960s quickly discovered that "Apathy was too easy a catchword"—it disguised more than it revealed, and thus misrepresented the underlying social reality. "To understand 'apathy' in its fullest meaning," Reese Cleghorn and Pat Watters therefore concluded:

> was to know that once all the South was totally resistant to the Negro vote, and that still none of the South was free of some form of resistance, and that just as the worst resistance of 1962–4 cast its shadow on areas where things were easier, so the *memory and shadow of all the past resistance would be some part of "apathy" in 1962–4 and, probably, for some time to come* . . . violence and intimidation not just last year but twenty years ago take their toll.[5]

Clearly, it is time to reconsider empirically the role of fear in political participation.

In addition to "apathy" and "fear," however, there is a third potential explanation of variations in political participation. An impressive body of social science literature links protest and agitation to the degree of discrimination a disadvantaged group experiences. The greater the discrimination, this literature suggests, the greater the likelihood of protest, particularly when a period of relative improvement creates expectations that are then frustrated by a period of stagnation or decline.[6]

Instead of a single "explanation" of political participation, in other words, there may be three: one based on *fear,* the second on *discrimination,* and the third on *apathy*. The purpose of this article is to evaluate these three potential explanations of political participation patterns by analyzing how well they account for the variations in political participation among blacks in the 29 black-majority counties of Mississippi during the first half-decade following the 1965 Voting Rights Act. This analysis will require the formulation of a suitable measure of political participation or mobilization, the translation of each of the three "explanations" or models into "operational" terms, and the application of an appropriate "test" to measure the explanatory power of each. Before turning to these matters, however, we must look briefly at the overall record of black political participation in the Deep South since 1965, for this record poses the problem the remainder of this article seeks to unravel.

The Problem: Variations in Black Voter Turnout

When Congress passed the Voting Rights Act in 1965, hopes ran high that the law would soon usher in a new day of democracy in the Deep South. With one surgical stroke, Congress cut away the elaborate legal growth that Southerners had nurtured for nearly a century to keep blacks from voting, and guaranteed that for five years the federal government would continue to root out any reappearances of the cancer.[7] "They came in darkness and they came in chains," President Johnson declared when he signed the law. "Today we strike away the last major shackle of those fierce and ancient bonds."[8]

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[4] Donald R. Matthews and James W. Prothro, *Negroes and the New Southern Politics* (New York: Harcourt, Brace & World, Inc., 1966), p. 308 (emphasis added).

[5] Pat Watters and Reese Cleghorn, *Climbing Jacob's Ladder: The Arrival of Negroes in Southern Politics* (New York: Harcourt, Brace and World, 1967), pp. 124–25 (emphasis added).

For other evidence from participant observers about the role of fear in southern black politics, see: Sally Belfrage, *Freedom Summer* (New York: Viking Press, 1965); Anne Moody, *Coming of Age in Mississippi* (New York: The Dial Press, 1968); Polly Greenberg, *The Devil Has Slippery Shoes: A Biased Biography of the Child Development Group of Mississippi* (London: The Macmillan Company, 1969); William McCord, *Mississippi: The Long Hot Summer* (New York: W. W. Norton and Co., 1965); James Silver, *Mississippi: The Closed Society* (New York: Harcourt, Brace and World, 1963).

[6] Tanter, for instance, found that domestic violence is greatest in countries where income inequality is greatest. Raymond Tanter, "Toward a Theory of Conflict Behavior in Latin America," paper prepared for delivery at the Annual Meeting of the International Political Science Association, 1967.

This discrimination factor corresponds very closely to the concept of "relative deprivation" formulated by students of domestic violence. In its more refined forms, however, the "relative deprivation" notion focuses not simply on the actual distribution of resources between the lower classes and their "betters," but also on the perceptions of the lower classes about what they have a right to expect and on the relationship between these expectations and the possibility of fulfilling them. Most empirical tests of this notion to date, however, have had to measure these aspirations indirectly. See: James Davies, "Toward a Theory of Revolution," *American Sociological Review,* 27 (February, 1962), 5–19; Harry Eckstein, "On the Etiology of Internal Wars," *History and Theory,* 4 (1965), 133–63; Ted Gurr with Charles Ruttenburg, *The Conditions of Civil Strife: First Tests of a Causal Model* (Princeton, N.J.: Center of International Studies, April 1967); Neil J. Smelser, *Theory of Collective Behavior* (New York: The Free Press, 1963). For an opposite view, albeit on a slightly different point, see Crane Britton, *The Anatomy of Revolution,* rev. ed. (New York: Vintage Books, 1952), especially pp. 264–65.

[7] The Voting Rights Act of 1965 was the culmination of extended efforts by civil rights forces to secure federal assistance in their efforts to eliminate the barriers to black political participation in the South.

From afar, the record of the first half-decade under this law seems to confirm this euphoria. In the South as a whole, black registration rose from slightly more than 2 million in 1964 to 3¼ million by the end of the decade, and the number of black elected officials jumped from 72 to 473. In Mississippi, the results were even more dramatic: black voter registration soared from a mere 28,500 prior to the Voting Rights Act to 281,000 five years later, while the number of black elected officials rose from zero to 72.[9]

Closer scrutiny, however, reveals a far less hopeful picture of black political progress. While the number of blacks registered to vote in the South rose by one million between 1964 and 1970, for example, the number of white registrants rose by two million, so that blacks ended the decade with little gain in relative power.[10] More important, black voter turnout has rarely exceeded 50 per cent of those eligible, except in the more urban counties. As a result, although 85 southern counties had black voting age population majorities as of 1960, only four have elected black sheriffs and only four have elected black majorities on their governing boards. In Mississippi alone, 29 counties have black majorities, yet none elected a black sheriff or a black majority on its governing board, and only one elected a black legislator in the first half decade under the Act.[11] Nor does this situation seem to be changing. In the recent November 1971 elections in Mississippi, 309 blacks ran for public office at either the state or county level. Of these, 259 lost, including 28 out of 29 candidates for state legislature, 10 out of 10 candidates for Chancery Clerk (county executive), 14 out of 14 candidates for Sheriff, 10 out of 10 candidates for county School Superintendent, and 67 out of 74 candidates for county Boards of Supervisors, even though virtually all of these candidates were running in districts with black majorities. The only victories, in fact, were in the least consequential offices—Justice of the Peace, Constable, etc.—while blacks running for more significant posts went down to defeat almost without exception.[12] Apparently, something other than the legal barriers removed by the Voting Rights Act is keeping blacks from participating fully in the southern political process.

Even more puzzling than the weakness of this overall performance, however, have been the variations in black political participation among counties. Consider the case of Holmes and Humphreys counties in Mississippi, for example. On November 6, 1967, the newly enfranchised black voters of Holmes County elected the first black in this century to the Mississippi House of Representatives in the first local elections after the Voting Rights Act. On the same day, blacks in adjoining Humphreys County did not even bother to run a candidate for political office, even though they comprised 70 per cent of the county's population, and even though the white candidates running were no more popular among blacks than those in neighboring Holmes County. Nor can this contrast be explained by any obvious difference between the two counties; for the two appear, on the surface, to be virtual twins. A visitor would notice that Holmes to the east is a bit more hilly, but both are overwhelmingly rural; neither contains a town exceeding 3,000 persons; both grow primarily cotton and soybeans on their fields; and in both the visitor would encounter black and white faces in almost exactly the same proportions (about 2½:1). Residents in both counties, moreover, are exceedingly poor (the median income was $1580 in Humphreys and $1453 in Holmes in 1960) and ill-educated (the median adult education was 6.2

The new law outlawed the literacy tests and other discriminatory voter registration procedures, authorized the Attorney General to appoint voting registrars in counties where less than half of the voting age blacks were registered, and created a procedure to review any changes in election law in five southern states, including Mississippi. *Voting Rights Act of 1965*, 79 Stat. 437, 42 U.S.C., 1973. The Act was extended in 1970 for another five years.

[8] Quoted in James Sundquist. *Politics and Policy: The Eisenhower, Kennedy and Johnson Years* (Washington, D.C.: The Brookings Institution, 1968), p. 274.

[9] Voter Education Project, *Black Elected Officials in the Southern States*, mimeo (1969). p. ii; *V.E.P. News*, Vol. 3, No. 7, July 1969. p. 1; *V.E.P. News*, Vol. 4, Nos. 1 & 2, January-February 1970, p. 3.

[10] *V.E.P. News*, Vol. 4, Nos. 1 & 2, January-February 1970, p. 3. In Mississippi, the black registration gain was more substantial: 242,500 black registrants were added compared to 147,000 whites.

[11] This information comes from the Voter Education Project, Atlanta, Georgia. The four counties with black sheriffs as of July 1972 were Greene, Lowndes, Macon, and Bullock—all in Alabama. The four counties with black majorities on their governing boards were Greene in Alabama, Hancock in Georgia, and Surrey and Charles City in Virginia. In addition, at this writing, blacks have a majority on the Election Commission in Holmes County, Mississippi.

[12] Millsaps College, The Institute of Politics in Mississippi, "General Election 1971: Some Notes on Elections Involving Black Candidates." mimeographed. For an analysis of this election, see: Lester M. Salamon, "Mississippi Post-Mortem: The 1971 Elections," *New South*, 27 (Winter 1972), 43–47.

years in Humphreys and 6.9 years in Holmes in 1960). Finally, the white communities of both counties have been equally unfriendly to black political aspirations. In both, blacks were totally excluded from the political process before 1965. And in one test of white racial feeling, the 1964 presidential election, an identical 96 per cent of the white voters in both counties bolted from the Democratic party and from the first southern candidate for president in memory in order to support a candidate who voted against the 1964 Civil Rights Act.[13]

Were this contrast between Holmes and Humphreys counties unique, it would naturally attract little interest. But it is not unique. The same puzzling contrasts occur between other seemingly identical counties in Mississippi—like Marshall and DeSoto, or Issaquena and Sunflower—as well as between similar counties elsewhere in the Deep South. While in some areas blacks have been able to take advantage of the opportunity afforded by the Voting Rights Act to build significant bases of political power, in others they have not, and the reasons for the difference are not immediately apparent.

These baffling variations in black political participation may, of course, be purely accidental. On the other hand, however, they may hold important clues about the surviving barriers to full black political participation in the Deep South, and, by implication, about the barriers to the participation of disadvantaged groups in politically underdeveloped areas elsewhere in the world. The remainder of this article seeks to determine which of these alternatives is correct by systematically examining the variations in black political participation rates in Mississippi in the light of the three models outlined above.

Methodology and Data

Two methods are potentially available to analyze political participation patterns: survey research and aggregate analysis. Survey research has the important advantage of avoiding the so-called "ecological fallacy," the problem of inferring individual behavior from aggregate data,[14] because it permits a researcher to explain individual-level decisions to participate or not to participate in terms of individual-level data. For the purposes of this research, however, the survey method had serious drawbacks that argued against its use. Not only would a questionnaire have been prohibitively costly, but also there was serious reason to doubt the trustworthiness of the data it would generate. One of the hypotheses we proposed to test, after all, was that nonparticipation is a result of fear. But using a questionnaire to measure the degree of fear experienced by blacks in rural Mississippi is like using an electron microscope to examine an electron: in both cases, the measuring instrument knocks the object to be measured out of sight. What is more, questionnaire data could not provide an answer to the question of why black political action was more successful in some *places* than others, at least not without an extremely large sample. Because of these serious inadequacies of the survey method for our purposes, and in view of the recent literature refuting earlier criticisms of the aggregate approach,[15] we adopted the ag-

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[13] Demographic data is from U.S. Department of Commerce, Bureau of the Census, *1960 Census of Population: General Social and Economic Characteristics,* PC (1)-26C (Washington, D.C.: U.S. Government Printing Office, 1960).

Voting statistics are from F. Glenn Abney, *Mississippi Election Statistics, 1900–1967* (University, Mississippi: The University of Mississippi Bureau of Governmental Research, 1968), p. 28.

[14] The classic statement of this problem can be found in W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review,* 15 (June, 1950), 351–57. See also, Austin Ranney, "The Utility and Limitations of Aggregate Data in the Study of Electoral Behavior," in *Essays on the Behavioral Study of Politics,* ed. Austin Ranney (Urbana, Ill.: University of Illinois Press, 1962), pp. 91–102.

[15] The literature on "ecological inference" has come virtually full circle since Robinson's statement of the problem in 1950. In a recent volume on ecological analysis, for example, Erik Allardt argues that preoccupation with the question of faulty inference has distracted attention from the more important question of the fruitfulness of various hypotheses. And in Allardt's view, the ecological approach may "facilitate fruitful causal interpretations better than the corresponding individual data, if such are available." Erik Allardt, "Aggregate Analysis: The Problem of its Informative Value," in *Quantitative Ecological Analysis in the Social Sciences,* ed. Mattei Dogan and Stein Rokkan (Cambridge, Massachusetts: The M.I.T. Press, 1969), p. 8.

Even more important, several scholars have identified ways to minimize the dangers of ecological inference without sacrificing the entire approach. These methodological clues have been incorporated in the research reported here. In a 1967 article in this *Review,* for example, W. Phillips Shively pointed out that the dangers of "ecological inference" can be minimized so long as "individuals have been grouped in such a way that their scores on the dependent variable are unrelated to the aggregation in which they fall, except indirectly through their scores on the independent variable." As will become clear below, the explanatory models used here meet this condition. See W. Phillips Shively, "'Ecological' Inference: The Use of Aggregate Data to Study Individuals," *American Political Science Review,* 63 (December 1969), 1186; and Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1965), pp. 95–126.

The dangers of "ecological inference" have been gregate approach here, using "ecological regression" analysis. Instead of focusing on the individual potential black voter as the unit of analysis, therefore, we focused on the potential black electorate in an entire county. The research task, then, was to explain variations in black political participation among Mississippi's 29 black majority counties in terms of the characteristics of the potential black electorates in these counties.[16]

The first step, naturally, was to devise an index of black participation for each county (the dependent variable). Until recently, students of black politics in the South have used voter registration as the basic participation measure because registration figures were the only ones kept by race.[17] In Mississippi, however, registration figures are not kept by race. What is more, the real power lies in the vote, so that registration figures can be seriously misleading. Registering without voting is an empty gesture at best: it gives the outward appearance of activity but leaves the distribution of power essentially unchanged.

In the present study, therefore, an effort was made to measure voting power directly. In particular, two measures of black voter participation were developed. First, statistics were collected on the votes received by all black candidates who ran for countywide elected office (either in the primaries or in the general elections) during the first half decade after the passage of the 1965 Voting Rights Act.[18] The votes of the top two black vote-getters in each county were then averaged, in order to reduce the influence of special circumstances or unusual personalities, and the average was divided by the black voting age population in the county to give an overall mobilization index. Counties without two black candidates for countywide office were assigned indices on the basis of the assumption that had black candidates run in these counties, they would have done no better, but also no worse, than the weakest candidate who did run in another county.[19]

Strictly speaking, of course, this "mobilization" index may not be a precise measure of total black voter turnout since some blacks undoubtedly voted for the white candidates. In the context of Mississippi in the latter 1960s, however, those black voters who did so were effectively voting against the Civil Rights Movement, since all but a handful of the white candidates were outspoken opponents of the Movement. In fact, in the few cases where white candidates indicated their support for black suffrage and solicited black votes, black candidates did not run against them.[20] If not a precise measure of black *turnout*, therefore, the vote totals of the black candidates nevertheless constitute an excellent index of black political *mobilization*, of black electoral turnout in support of the Civil Rights Movement. This is particularly true, moreover, because the tense climate of Mississippi racial politics in the 1960s made it highly unlikely that very many white votes escaped to these black candidates, except by accident. The votes for these black candidates thus probably reflect quite well the number of blacks in each county who turned out to vote for a change in the status quo.

As a check on this mobilization index, how-

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reduced even further here by adopting the suggestion made recently by Dogan and Rokkan to "develop statistical models for the exploration of *alternative linkages* between individual distributions and higher unit characteristics" as a way to avoid the "ecological fallacy." In this article, three "alternative linkages" are explored. See Mattei Dogan and Stein Rokkan, "Introduction," *Quantitative Ecological Analysis*, ed. Dogan and Rokkan, p. 8 (emphasis added).

[16] The county was chosen as the unit of analysis because it is the smallest unit for which political, economic, and social statistics are readily available.

Only the counties with black majorities were used because they are, by and large, the only ones where black candidates have run for countywide office and where civil rights activity has occurred, both of which are important in measuring the dependent variable, as will become clear below. Only the black majority counties in one state are used to eliminate the effects of different state voting laws and political histories. Mississippi was chosen because it is the state with which we are most familiar and because, except for Georgia, it is the only one with a large enough number of black majority counties to permit a statistically significant test.

The "explanations" developed here were tested using correlation and multiple regression analysis, two standard statistical measures of relationship. The data analysis was performed on the Vanderbilt University Sigma-7 computer using standard Product Moment Correlation and Multiple Regression programs. We are indebted to Laird Heal of the Vanderbilt University Computer Center for assistance with these programs.

[17] See, for example, Donald R. Matthews and James W. Prothro, "Social and Economic Factors and Negro Voter Registration in the South," *American Political Science Review*, 57 (March, 1963), 24–44, especially p. 26.

[18] Countywide offices were at stake in two Mississippi elections during this period—in 1967 and 1968. The posts for which candidates ran county-wide in 1967 were: Sheriff. Chancery Clerk, State Senator, and School Superintendent. In 1968, they were: Election Commission (five members per county) and School Board (five members per county).

[19] Four of the 29 counties analyzed here had only one black candidate for countywide office, and nine had none.

[20] This was the case, for example, with regard to the white candidate for Chancery Clerk in Wilkinson County, and the white candidate for State Senator in Holmes County.

Table 1. Index of Black Voter Mobilization by County, Twenty-Nine Mississippi Counties

County	Black Voter Mobilization Index	County	Black Voter Mobilization Index
Jefferson	53%	Leflore	29%
Claiborne	50	Sharkey	29
Wilkinson	49	Washington	25
Holmes	43	Bolivar	26
Issaquena	43	DeSoto	21
Jefferson Davis	43	Humphreys	20
Marshall	43	Jasper	19
Amite	42	Noxubee	19
Madison	40	Quitman	19
Copiah	39	Tate	19
Panola	34	Yazoo	18
Coahoma	30	Tallahatchie	17
Clay	30	Kemper	16
Carroll	30	Sunflower	15
		Tunica	15

ever, a second one was constructed on the basis of the votes in the 1968 presidential election, for which uniform returns are available for all counties. From the point of view of measuring black political participation, the 1968 presidential election provided an unusual opportunity. Where the 1964 presidential election had required southern whites to leave the Democratic party and vote for an arch-conservative non-southerner over a semi-Populist southern Democrat in order to register their opposition to civil rights, the 1968 election afforded them a greater array of more palatable alternatives. Both Nixon and Wallace appealed to the southern white vote. The Democratic candidate, on the other hand, was the very northerner whose civil rights stand at the 1948 Democratic election prompted the southerners to storm out in protest. If Lyndon Baines Johnson was able to attract no more than 5 or 6 per cent of the white votes in these black-belt counties in 1964, therefore, it seems likely that Hubert H. Humphrey received far fewer in 1968. As a result, the Humphrey vote totals very probably represent the clearest possible approximation of the black voter turnout in this election, or at least of the turnout of blacks who support the struggle for civil rights.

Since these two indices of black voter mobilization turned out to be very closely related (correlation = +.924), a single, combined index was computed by taking an average of the two.[21] The results, noted in Table 1 above, demonstrate the wide variation in black political mobilization among Mississippi counties.

The task now is to try to explain these variations, using the three models of participation mentioned earlier. To do so, it is necessary to translate each model into operational terms and test it against the data on black political participation. The following three sections treat the fear, discrimination and apathy models in turn, showing how each model was measured and what proportion of the variation in black participation rates it is able to account for. In each case, the Humphrey vote, the black candidate vote, and the combined black mobilization index were all tested separately. Since the results were virtually identical, however, only the results using the combined index are reported.

Fear and Black Political Participation

Fear is an exceptionally difficult phenomenon to measure, which helps to explain why students of political participation have generally ignored it. It may also help to explain why Matthews and Prothro, the only recent students of political participation who have analyzed the role of fear, reached a conclusion that differs so fundamentally from virtually all the first-hand, participant-observer reports available about black politics in the South in the late 1950s and early 1960s.[22]

Matthews and Prothro measured the impact of fear in two ways: first, by comparing participation rates to the incidence of overt racial vio-

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[21] In mathematical terms, this participation index was computed as follows:

$$\text{Country Black Participation Rate} = \frac{\dfrac{\text{Humphrey Vote}}{\text{County Black VAP}} + \dfrac{\text{Vote for top two black candidates for countywide office/2}}{\text{County Black VAP}}}{2}$$

[22] See note 5, supra.

lence in southern counties (as measured by the Tuskegee lynching statistics and the more recent Southern Regional Council's statistics on civil rights violence); and second, by relating participation rates to questionnaire responses from blacks who were asked if they "had ever heard of anything happening to Negroes around here who have voted or taken some part in politics or public affairs."[23] In both cases, they hypothesized that participation would be low where the incidence of violence, or the perception of violence, was high.

Both of these measures have serious drawbacks, however—so serious, in fact, that they throw into question Matthews's and Prothro's whole conclusion that "intimidation and fear" are "not major causes" of Negroes' failure to participate in politics as much as whites.[24] In the first place, by hypothesizing an inverse relationship between violence and participation, Matthews and Prothro misread seriously the role of violence in southern society, and the relationship between violence and fear. In particular, they assume that the absence of violence signifies the absence of fear. In the rural Deep South, however, the opposite seems closer to the truth. As the United States Civil rights Commission pointed out as late as 1968, a "generalized atmosphere of intimidation" prevailed in many areas of the Deep South "even in the absence of specific threats or reprisals."[25] Historically, violence in the Deep South has been the instrument-in-reserve—perpetually available, but not always used. Violence, after all, is an expensive form of social control, and those in positions of influence have every incentive to restrict its use.[26] The Southern caste system was most successful, therefore, when it was least violent—i.e., when the *fear* of violence made the *use* of violence unnecessary. To equate the nonoccurrence of violence with the absence of fear, as Matthews and Prothro do, is thus to misinterpret badly the underlying social reality. Indeed, far from expecting the most violent places to be the *least* active politically, there is reason to expect them to be the most active; for violence in the rural South has historically not been unleashed without a cause, and that cause has usually been black unrest and agitation. Matthews and Prothro, in other words, may be confusing cause and effect. Fear may be the cause of low participation; but the presence of overt violence may really signify the breakdown of fear.

A second problem with Matthews and Prothro's treatment of fear is that by focusing exclusively on overt violence, it ignores some of the more important, day-to-day sources of fear. Most ubiquitous of these is economic coercion. While violence, or the fear of violence, may have created the desired climate of apprehension, economic intimidation has usually provided the more reliable and economical instrument of control. In the rural and small town setting of the Deep South, most blacks could be controlled through the simple expedient of economic pressure without resort to physical violence. This was so because virtually all blacks were dependent to some extent on local whites for their income, and because the small-town setting facilitated intimate social control. If a black became "uppity" and began demanding his "rights," it was easy to organize a comprehensive program of coercion: to have him fired, deprived of a loan at the bank or store, barred from receiving welfare, and harassed by the authorities. Violence could therefore be reserved for the unusual cases, while economic pressure did its job day by day. When one national civil rights group developed a standardized form for checking off the excuses local Negroes gave for refusing to register to vote, therefore, it discovered that "economic pressure" easily led the list of items checked.[27] And this was so not only for the poverty-stricken plantation laborers and maids, but also for the better-off teachers and professionals who worked for white-dominated county School Boards. A study commissioned by the U.S. Civil Rights Commission in 1964 to determine why so few black teachers registered to vote in Mississippi thus discovered that "fear of loss of job was the most common response," outdistancing even "fear of violence."[28] On the basis of this and other evidence, the Civil Rights Commission concluded that "the economic dependence of Negroes in the South inhibits them from engaging freely in political activity and voting for candidates of their choice."[29] By focusing exclusively on physical violence and ignoring the relationship between economic intimidation and fear, Matthews and Prothro leave themselves open to serious questions about the credibility of their conclusion that fear is not a cause of black nonparticipation in politics.

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[23] Matthews and Prothro, *Negroes and the New Southern Politics,* pp. 166 and 303.

[24] Matthews and Prothro, p. 308.

[25] U.S. Commission on Civil Rights, *Political Participation: A Study of the Participation by Negroes in the Electoral and Political Processes in Ten Southern States Since the Passage of the Voting Rights Act of 1965* (Washington: U.S. Government Printing Office, 1968), p. 127.

[26] For a revealing discussion of the role of violence and intimidation of blacks in the southern caste system, see: Allison Davis, Burleigh B. Gardner, and Mary R. Gardner, *Deep South: A Social Anthropological Study of Caste and Class* (Chicago: University of Chicago Press, 1941), pp. 392–400.

[27] Watters and Cleghorn, *Climbing Jacob's Ladder.* Watters and Cleghorn report (p. 129) that Mississippi bankers used to tell black loan applicants in the 1960s that: "If you can afford to vote, you don't need a loan."

[28] U.S. Commission on Civil Rights, *Hearings,* Jackson, Mississippi, February 16–20, 1965 (Washington. D.C.: U.S. Government Printing Office, 1965), I, 209. Interestingly, this study was done by James Prothro, yet Prothro neglected to mention it in his book on Negro politics in the South published two years later.

Since violence is a poor index of fear, some other measure had to be found. The discussion above, and the general literature on southern society, suggested a solution to this problem by making it clear that fear in the rural South is not so much a result of specific incidents of repression as it is a result of a general condition of *vulnerability*. To the extent that the level of fear among blacks varies from place to place, therefore, it should vary in relation to the degree of black vulnerability to white coercion—whatever the form of that coercion. The search for a suitable measure of fear thus resolved itself into a search for suitable measures of black vulnerability. Two such measures seemed most promising, moreover—one reflecting the degree of black *economic dependence,* and the other the level of black *organizational development* in a county.

Economic Dependence and Participation: The "Simple Fear Model." So far as economic dependence is concerned, the discussion above, and the general literature on the southern caste system, suggest quite strongly that blacks in the Deep South are likely to be less fearful to the extent that they are not economically dependent on local whites and are therefore able to withstand at least a modicum of economic pressure. Economic dependence, however, does not seem to be merely a function of income. After all, the wealthiest class of blacks, the school teachers, are commonly acknowledged to be among the most dependent, for their income is controlled by white school officials who have not hesitated to keep the threat of expulsion constantly before potentially "uppity" black educators.[30] By contrast, black farm *owners,* although generally far poorer, at least enjoy a modicum of economic independence. Not the *amount* of income, but the *source* therefore seems most crucial. Thus, the larger the proportion of blacks in a county who receive their incomes from sources relatively independent of local white control, presumably the greater the black political participation rate.

To test this idea, the U.S. Census' occupational classifications were divded into two basic groups according to how vulnerable the blacks holding these occupations would be to white economic coercion. Black farm owners and businessmen, for example, were placed in the "least vulnerable" category, and plantation laborers and maids in the "most vulnerable." (See Table 2 on the next page.) The number of blacks in each category was then computed as a percentage of the black voting-age population for each country. This figure became the basic index of vulnerability and formed the basis of what we will call the "simple fear model" of black participation.[31]

As a check on this occupation-related economic dependence figure, however, data were collected on another measure of black economic vulnerability—home ownership. The frequent stories of civil rights activists who were evicted from their homes in retaliation for registering to vote suggested quite strongly that black families who own their homes will feel less vulnerable, and hence less fearful, than

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[29] U.S. Commission on Civil Rights, *Political Participation,* p. 127.

[30] The most systematic evidence on the point is the four-county survey of black Mississippi school teachers conducted for the U.S. Civil Rights Commission. Commissioner Rankin summarized this study well when he asked the primary investigator if low teacher voter participation could not be attributed to "the advice of the superintendents and principals that 'you be careful or you might lose your job. . . .' " The answer was an unequivocal yes. U.S. Commission on Civil Rights, *Mississippi Hearings*, 1965, I, pp. 207–215, especially p. 214.

[31] It is important to note here that Matthews and Prothro, in their study of Southern black political participation, found little relationship between black occupational level and black political participation. However, Matthews and Prothro were content to divide occupations simply into the standard white-collar vs. blue-collar categories commonly used by sociologists and political scientists. In doing so, however, they were implicitly accepting a conceptual framework we have argued is inadequate to bring the key features of black politics in the South into focus. Matthews and Prothro, *Negroes and the New Southern Politics,* pp. 12–13.

Dividing the black labor force into "most vulnerable" and "least vulnerable" categories, however, is not a simple task. The Census classifications of occupations do not correspond very precisely to the distinction of interest to us here. For example, the occupations grouped together in the Census classification "Operatives and Kindred Workers" include highly vulnerable plantation tractor drivers and far less vulnerable railroad employees. The Census classification "Farmers and Farm Managers" includes both farm-owners and sharecroppers. To compile Table 2, therefore, it was necessary to make a number of difficult decisions, reflected in the Table notes. These decisions were based on the existing sociological literature on southern society, and on numerous interviews with civil rights activists in Mississippi.

Table 2. Occupations of Nonwhite Labor Force Arranged According to Vulnerability to White Economic Pressure

Least Vulnerable	Most Vulnerable	Uncertain
Farmowners[a]	Farm Laborers	Clerical Workers
Manufacturing Workers[b]	Sharecroppers and Tenants	Sales Workers
Non-Farm Proprietors	Household Service Workers	Craftsmen
Workers Employed Outside County of Residence[c]	Unemployed	Operatives
Professionals[d]		Service Workers, Except Private Household

[a] Instead of using the Census of Population classification "Farmers and Farm Managers," which includes tenants and sharecroppers, the number of black farmowners was taken from the *U. S. Census of Agriculture*, 1964. This created some incomparability in the figures, but not enough to make a difference.

Farmowners were considered relatively *in*vulnerable because they could at least not be fired, and because they normally grew some of their own food, thus making them able to resist at least some economic pressures.

[b] Instead of trying to cull the number of manufacturing workers from the occupation classifications of "Craftsman," "Operatives," and "Non-Farm Laborers," the *industry* classification "Manufacturing Workers" was used instead of the *occupation* classifications.

The decision to include manufacturing workers among the Least Dependent reflected the assumption that it is difficult to impose sanctions in an industrial enterprise because of the scale of the operation and the problem of training replacements. Manufacturing employers, moreover, were likely to be outsiders who would be less likely to impose economic sanctions on black employees who might vote.

[c] The decision to include "Workers Employed Outside their County of Residence" in the Least Vulnerable category reflected the testimony of civil rights activists that whites have greater difficulty coercing blacks who have jobs in other counties.

Nevertheless, the inclusion of this factor caused some unavoidable overlap. Many of those who worked outside their county of residence were probably manufacturing workers, but there is no way to determine how many.

[d] Inclusion of the "professionals" in the Least Vulnerable category was perhaps the most difficult decision of all. The largest group of professionals, the public school teachers, are hardly among the "least dependent." But other professionals, including doctors, lawyers, embalmers, federal government employees, and some preachers enjoy relatively great independence. The inclusion of the "Professionals" in the Least Vulnerable group rested on the assumption that the *percentage* of the nonwhite labor force employed as public school teachers would be roughly the same in each county so that any variations in the percentage of professionals in the county's nonwhite labor force could be attributed to the presence of professionals *other* than public school teachers.

those who are renting or occupying plantation shacks at the sufferance of the planter.[32]

These measures of economic dependence were then tested against the data on black political mobilization. The results, depicted in Table 3, give ample support to the hypothesis that black voter turnout would be lower in counties where blacks are most vulnerable to economic intimidation. The correlation between dependence and voter turnout was high, statistically significant, and in the expected direction, no matter which index of dependence was used. As Figure 1 illustrates, voter turnout tended to be highest in those counties with the largest percentage of blacks in the least dependent occupations, precisely the pattern hypothesized. This "simple fear model," in fact, accounts for 25–30 per cent of the variation in black political participation among Mississippi counties.[33] Without a sizable cadre of blacks with relatively secure sources of income, the evidence seems to confirm, a county is unlikely to enjoy a high rate of black support for the Movement, despite the legal safeguards of the Voting Rights Act. The right to vote in the rural South, in other words, still carries a rather substantial price for blacks.

Organization, Economic Dependence, and Black Participation: The "Expanded Fear Model." Critical as economic independence seems to be in explaining black voter turnout, however, it hardly tells the whole story. As Figure 1 illustrates, Jasper and Kemper Counties, with the same proportion of blacks in dependent occupations as Amite and Jefferson

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[32] Homeownership figures came from the *U.S. Census of Housing*, 1960, Vol. I: States and Small Areas, Part 26, Mississippi (Washington, D.C.: U.S. Government Printing Office, 1963), pp. 38–44. Significantly, these two measures of black economic dependence were closely related. The correlation between the percentage of a county's black electorate in the *Most* Vulnerable occupations, and the percentage of blacks in owner-occupied homes in a county was −0.863.

[33] The probability that this result might have happened purely by chance was a mere 0.0029, or only 3 in 1000. Using a 5 per cent level of significance, this result is therefore clearly significant.

Strictly speaking, the significance level or probability measure is not really applicable here because these counties are not perceived as a random sample from a larger population. Nevertheless, the significance level is still interesting as a benchmark.

Table 3. Correlations Between Indices of Black Economic Dependence and Black Political Participation, Twenty-Nine Mississippi Counties

Index of Vulnerability	Correlation Coefficient
Percentage of NW labor force in most dependent occupations[a]	−0.538*
Percentage of NW labor force in least dependent occupations	+ .518*
Black homeowners as percentage of black families	+ .414*

[a] NW = Nonwhite.

* Significant at the .05 level.

Counties, nevertheless have black participation rates that are not even half as great. Blacks in Issaquena are more vulnerable economically than their brothers in Noxubee and Yazoo, yet support the Movement twice as much. In fact, it is possible to divide most of the twenty-nine counties into two groups, as indicated in Figure 1. Group A counties perform as expected in our hypothesis: participation increases as the percentage of persons in relatively independent occupations increases. Group B counties, however, fail to conform to this pattern. For them, changes in the degree of dependence seem to make *little difference* in voter turnout rates. Obviously some other factors are at work. Economic independence may be a *necessary* condition for black participation, but it is *not sufficient*. While variations in the degree of black economic dependence account for 25–30 per cent of the variation in black voter turnout, they still leave 70–75 per cent unaccounted for. How can we account for this remaining variation? And does it discredit the view that "fear" is the cause of black nonparticipation?

A glance at the recent history of just one of the counties represented in Figure 1, Holmes County, suggests a possible answer to these questions. Blacks in Holmes County were no less economically dependent on local whites in 1967, when they elected the first black man in a century to the Mississippi Legislature, than they were in 1962, when fewer than a handful of blacks were willing to try to register to vote. What transpired between these dates, moreover, was not a change in white attitudes, but the emergence of a grass-roots, black political orga-

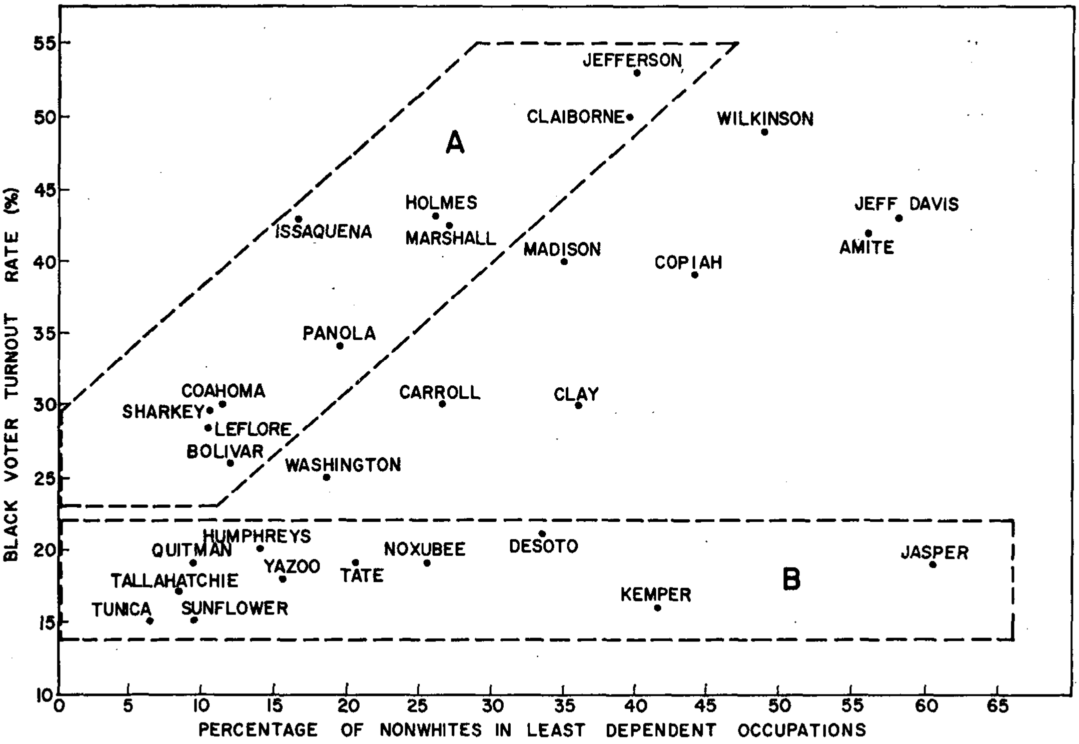

Figure 1. Comparison of Black Voter Turnout and Percentage of Non-White Voting Age Population in the "Least" Dependent Occupations in 29 Mississippi Counties.

nization. According to the two civil rights organizers who worked in the county during this period, the major consequence of this organizational development was to enable individuals to "manage their fear" through group solidarity.[34] What this suggests is that fear, at least fear of violence, can be mitigated by *organization*, by the creation of explicit bonds of trust that enable sizeable groups of individuals to coordinate their action and thus spread whatever risks flow from political involvement.

Given the conventional explanation that links organization to political participation by reference to the effect political organization has on reducing apathy and stimulating interest,[35] it is important to inquire why this might be so, why organization should be linked to participation through the medium of fear as well. The answer, we hypothesize, lies in the cost-benefit logic that confronts any individual contemplating political action in a context that has traditionally rewarded such action with violent reprisal. In the absence of organization, there is no way to share risks by orchestrating coordinated action by many. As a consequence, no one wishes to act, since the individual risks are too great. Furthermore, the very process of building an organization which would permit mass action will probably never develop spontaneously, because to begin it would require a very risky initiative on someone's part. So everyone is paralyzed—no one dares to move. But if organizations can be forged, some of this fear-induced paralysis can be relieved. If a thousand people march through the county seat one morning, the risk run by each marcher is only one in 1,000, while it would be closer to one in one for the first solitary picket or voter registrant in an unorganized context. As the number of people taking action grows, the risk being run by each individual falls until a point is reached where the risk is acceptable, people will run it, and the paralysis is broken.

This hypothesized inverse relationship between the deterrent effect of reprisals and the amount of organization in the victim community has limits, of course. It assumes, first, that reprisals are in fact accurately directed at individual political activists or at least believed to be in the victim community. But it also assumes that there is some relatively low ceiling on the scope of these reprisals such that acts of violence cannot in practice be extended to all members of an organization as it grows. Both of these conditions seem to have been met in Mississippi during the 1960s, where violence was normally aimed at the blacks who first ventured into political action, and where the potential presence of the national news media and the FBI kept the absolute level of violence in check. Even so, there is no suggestion here that organization can be as effective an antidote to *fear of economic coercion* as it can be to *fear of physical violence.* Unlike physical violence, economic coercion is more readily hidden from national attention and hence has no practical ceiling on its use. Organization cannot cure all fear, therefore, particularly in the absence of economic independence. Finally, by suggesting that organization affects participation by lessening fear, we do not intend to suggest that it does not also have its other, more traditional kinds of impacts as well. However, the experience of Holmes County and two other counties studied intensively by one of the authors (LMS), as well as numerous interviews with civil rights activists in the South suggest quite strongly that the major consequence of organization was to relieve fear, particularly fear of violence. Our hypothesis, then, is that the level of fear in a county, and hence the level of black political participation, should vary with the degree of black organization, other things being equal.

To test this hypothesized relationship between organization, fear, and political participation, it was necessary to develop a measure of black organizational development for each county. Since intensive organizational analysis of all 29 counties was practically impossible, however, we measured organizational development indirectly, by determining the extent to which certain crucial noneconomic *facilitators* of black organizational development were present in each county. (This approach had the added advantage of making it possible to avoid tautologies by making it unnecessary to measure the extent of organizational development in terms of the presumed *effects* of the organizations.) A detailed examination of Movement development in several Mississippi counties by one of the present authors (LMS) and discussions with numerous Movement leaders identified three such noneconomic facilitators of black organizational development: (1) the absolute size of the county's black population; (2) the percentage of the country's voting age population that is black; and (3), the extent of assistance from "outside organizers" to help communities take the first steps in managing their fear.

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[34] Sue and Henry Lorenzi, "Managing Fear by a Community—Holmes County, Mississippi," mimeo. The Lorenzi paper suggests that blacks in the rural South never *lose* their fear. Rather, they learn to *manage* it by participating in group activity that threatens the two-caste system. For a fuller analysis of the dynamics of black organizational development in three Mississippi counties in the 1960s, see Lester M. Salamon, *Backwardness and Change in the American South: Mississippi as a Developing Society* (Bloomington: Indiana University Press, forthcoming 1974), chapter 5.

[35] William J. Crotty, "Party Effort and Its Impact on the Vote," *American Political Science Review*, 65 (June 1971), 439–50; John C. Blydenburgh, "A Controlled Experiment to Measure the Effects of Personal Contact Campaigning," *Midwest Journal of Political Science*, 15 (May 1971), 365–81; David E. Price and Michael Lupter, "Volunteers for Gore: The Impact of a Precinct-Level Canvass in Three Tennessee Cities," *The Journal of Politics*, 35 (May 1973), 410–38.

The first two of these facilitators of fear management and organization are relatively straightforward. Because organization is essentially a matter of trust and communication, it is naturally easier to organize a county if there are fewer people to reach. Similarly, it is easier to inspire confidence and induce people to take risks when you have a 4:1 voting majority than when you have a slim 3:2 majority. Moreover, both of these factors are easy to measure from readily available Census materials.

The role of outside organizers, however, is much more complex and much more controversial. Between 1961 and 1965, hundreds of young blacks and whites trooped to Mississippi under the sponsorship of SNCC and the Council of Federated Organizations (COFO),[36] and fanned out into the interior of the state to help local people organize themselves for political action. In addition, throughout this period the NAACP maintained in Mississippi a "field director" who attempted to organize black communities throughout the state.[37]

In the wake of the "black power" movement of the mid-1960s, the contribution these "outside organizers" made to the development of black political organizations in Mississippi has been seriously questioned. Yet these "outsiders" had certain unique strengths that made it possible for them to play an important role in getting organizational development started. For example, the outsiders had connections with the national press that insulated them in part from the more violent forms of repression. They did not have to worry about economic reprisals since they had no families in the area and were not dependent on local whites for their livelihood. They could therefore take greater risks than could local blacks. They were able to act as catalysts, taking the first critical steps to get organization started and absorbing the blame for the earliest, and most dangerous, Movement activity. What is more, they possessed a certain amount of "technical information" about how to conduct a meeting, about what other groups were doing, about what the laws said. Finally, they frequently provided a "shortcut" for individuals who wavered between commitment to the Movement and fearful inactivity. The mere presence of white students in particular created a dilemma for local blacks because of the sharp conflict between the rural norm of hospitality and the caste proscriptions on social contact with whites. When a cherubic white student "dropped by" to talk about voter registration, few black families could deny their heritage enough to treat the student inhospitably. The result was to induce many people to "cross the line," to defy the caste code, but to do so relatively effortlessly.

Significantly, outside organizer assistance was not distributed evenly among Mississippi counties. Some counties received heavy doses, particularly during the famous "Mississippi Summer" of 1964, while others were virtually ignored. To determine what level of organizer assistance each county received, eight persons with long experience in the Movement in Mississippi were asked to assign a score from 1–5 to each of the twenty-nine black majority counties in the state, according to the amount of "outside organizer" assistance the county had received during the years 1960–68. The scores from each interviewer were then added to give an overall "organizer score" for each county.[38] Together with the measures of population size and the proportion of blacks in a county, this "outside organizer" score provides a way to assess the likelihood that a county has succeeded in managing its fear through organization. Together with the measure of *economic dependence* examined earlier, these three measures of *organ-*

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[36] COFO was the coalition of civil rights groups formed in 1963 to sponsor voter education projects throughout Mississippi and to coordinate the black political battle in the state. At the height of COFO activity, in the historic summer of 1964, some 500 outside workers, mostly white students from the North, were in the state. "COFO files," Delta Ministry Office, Mt. Beulah, Edwards, Mississippi. See Watters and Cleghorn, *Climbing Jacob's Ladder,* pp. 63–68; Belfrage, *Freedom Summer.*

[37] Until his murder in 1963, the NAACP Field Director in Mississippi was Medgar Evers. Soon after this, Medgar Evers's brother, Charles Evers, took over this job and served until 1969, when he was elected Mayor of Fayette, Mississippi.

[38] The eight interviewees were: the Reverend Harry Bowie, originally a SNCC volunteer, who works now for the Delta Ministry in McComb, Mississippi; Lawrence Guyot, former state chairman of the Freedom Democratic Party; Aaron Henry, President of the Mississippi NAACP and co-director of COFO; Jan Hillegas, Editor of the Freedom Information Service Newsletter, a Movement publication; Charles Horowitz, Delta Ministry staff member; the Reverend Edwin King, former chaplain at Tougaloo College and Movement candidate for Lieutenant Governor in 1963; William F. Minor, Jackson correspondent for the New Orleans *Times-Picayune.*

Table 4. Test of the "Expanded Fear Model:" Impact of Four Fear-Related Factors on Black Political Participation in Twenty-Nine Mississippi Counties

Factor	% of Variation in Black Turnout Accounted for by Addition of this Factor (Partial r^2)	% of Variation in Black Turnout Accounted for by this Factor and All Above it (Multiple R^2)
Black Economic Dependence	28.9%**	28.9%**
NW Voting Age Population	0.6**	29.5**
Percentage of VAP Nonwhite[a]	17.0**	46.5**
Outside Assistance	22.1**	68.6**

[a] VAP = Voting Age Population.
** Significant at the .01 level.

izational potential provide an overall index of vulnerability and fear for each county. The combination of these four, fear-related factors constitutes what we call the "expanded fear model" of black participation.

When this model is tested against the data on black political participation using multiple regression analysis, the results are striking. As Table 4 above indicates, variations in the absolute size of the nonwhite population in a county seem to add little to the explanation of variations in black participation already achieved by the "simple fear model" (partial $r^2 = 0.6$ per cent), but the other two measures of organizational potential—and hence of fear mitigation—contribute substantially indeed. In fact, taken together, these four, fear-related factors that comprise the "expanded fear model" account for almost 70 per cent of the variation in black participation rates. The probability that this result could have happened purely by chance, moreover, was so slight that it could not be computed. (See Table 4.)

Here, then, is powerful support for the argument that fear plays a vital role in black politics in Mississippi. This finding directly challenges the euphoric hope that the Voting Rights Act of 1965 eliminated, in President Johnson's words, the "last major shackles" impeding black political liberty in the South. The evidence suggests quite powerfully that the heritage of exploitation and repression, sustained by persistent economic dependence and coercion, may be doing to the Voting Rights Act of 1965 what it did to the Emancipation Proclamation a century before: transforming it into a legal form lacking social content.

Before we rest content with this conclusion, however, it is necessary to consider one further matter. To attribute variations in black political participation to variations in the degree of vulnerability of black communities is to assume that counties do not vary greatly in their treatment of blacks, or at least that these variations do not significantly affect turnout. The measures of fear used so far, after all, related only to the black communities themselves. Is it not possible that variations in the way whites treat blacks might affect turnout rates also?

To answer this question, it was necessary to develop an index of white hostility. The measures selected relied chiefly on voting statistics, since these statistics are most reliable and are available uniformly for all counties. In particular, white hostility to black power in a county was measured by the extent to which the county preferred Goldwater over Johnson in the 1964 presidential election, and the extent to which its voters turned out to support Wallace in the 1968 presidential election. Although indirect, these two measures do tap the degree of active white opposition to change in each of the 29 counties.[39]

When these measures of hostility were tested against the data on black participation, the results suggested that variations in the degree of white hostility from county to county do not significantly affect black political participation. The correlations between support for Goldwater and for Wallace, the two indices of white hostility, and black voter turnout were too small to be significant. Indeed, what little relationship there was between white hostility and black political participation was in the wrong direction: instead of decreasing, black participation tended to *increase* with increases in white hostility to black rights. (See Table 5.) Moreover, adding these hostility factors to the "expanded fear model" presented above contributes less than one-tenth of one per cent to the amount of variation accounted for.

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[39] The Goldwater vote was computed as a percentage of the two-party vote, instead of as a percentage of the potential white electorate, because, in 1964, no real threat to white political dominance existed and whites therefore felt little need to turn out for the election. A measure of white turnout, therefore, would have been meaningless, whereas a measure of county preference for Goldwater over Johnson had social relevance in view of Johnson's support for social welfare and civil rights legislation.

By 1968, the presence of a real black challenge at the polls put a premium on white turnout. Not surprisingly, therefore, white turnout increased sharply. In 1964, both presidential candidates together received 409,146 votes; in 1968, Nixon and Wallace alone received 502,146 while Humphrey, drawing mainly on black voters, received an additional 149,419. In the 1968 election, therefore, the best measure of white hostility to black rights was the turnout of voters for Wallace as a percentage of the potential white electorate, for this measure permits us to determine the extent to which the white community in a particular county mobilized itself to express its conservative opinion.

These findings are consistent with the Matthews and Prothro findings that white violence—another measure of hostility—does not significantly influence black participation.[40] As already noted, however, the conclusion to be drawn from these findings is not that fear is not a key determinant of black participation, but rather that fear has a long half-life, that it persists even in the absence of specific incidents of violence or expressions of hostility. The variations in white hostility from county to county in the Mississippi black belt are simply not salient enough to give local blacks confidence that they are safer in some areas than others. Not the intensity of local white hostility, but the strength of the local black community is thus the key to dissipating fear and hence encouraging political participation.[41]

Altogether, then, these findings establish a *prima facie* case that fear is the cause of low turnout among black voters in Mississippi. They do not yet demonstrate, however, that the alternative models developed above, based on "relative deprivation" and "apathy," are incorrect. Indeed, it is even possible that our presumed measures of vulnerability and fear are really disguised measures of relative deprivation and apathy. Before we can have real confidence in the "expanded fear model" of nonparticipation, therefore, it is necessary to see how well the other two "models" explain the observed variations in black voter turnout among Mississippi counties.

Table 5. Correlations Between Two Measures of White Hostility and Black Participation Rates in Twenty-Nine Mississippi Counties

Measure of White Hostility	Correlation with Black Participation
Goldwater Vote as % of Total Presidential Vote, 1964	+.202 (n.s.)[a]
Wallace Vote as % of White Voting Age Population	+.240 (n.s.)[a]

[a] n.s. = not significant at the .05 level.

Table 6. Correlations Between Two Measures of Discrimination Against Blacks and Black Political Participation in Twenty-Nine Mississippi Counties

Measures of Discrimination	Correlation with Black Participation
Nonwhite Median Family Income as a Percentage of White	−.102 (n.s.)
Nonwhite Median Education as a Percentage of White	+.362 (n.s.)

Discrimination and Black Political Participation

The "discrimination" model contends that variations in black political participation are the result of variations in the degree of discrimination blacks experience locally. The greater the discrimination, the model suggests, the greater the political participation.

Two measures of inequality were developed to test this model, one focusing on income and the other on educational opportunity. In particular, for each county, the black median family income and the black median education level were computed as percentages of the corresponding white figures for 1960. These indices provided a direct measure of the degree of deprivation a county's black citizens experience vis-à-vis the white citizens of the same county.

According to the "discrimination model," black participation should be highest in those counties where discrimination is greatest (i.e., where income and education levels of blacks are lowest *relative to those of local whites*). In fact, however, discrimination accounts for very little of the variation in black participation rates, whether discrimination is measured in terms of relative income levels or relative educational levels. Although the correlation coefficient in the case of educational deprivation was sizable, it was not in the expected direction. (See Table 6) Participation was higher in counties where relative educational deprivation was lowest. Finally, neither of these discrimination factors added significantly to the explanation of black political participation already achieved by the four fear-related factors included in the "expanded fear model."[42] Clearly, discrimination, at least as measured by these two factors, adds little to our understanding of the reasons for variations in black political participation among Mississippi counties.

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[40] Matthews and Prothro, *Negroes and the New Southern Politics*, p. 308.

[41] The fact that we discover a positive, if slight, correlation between white turnout and black mobilization suggests that what we are really witnessing here is a white response to the threat of black mobilization. The more blacks become mobilized, the more whites turn out to vote. This pattern is consistent with findings dating back to V. O. Key's *Southern Politics in State and Nation*, particularly pp. 513–516. It also conforms to the finding of Matthews and Prothro that there is a slight *positive* correlation between the incidence of violence and the level of black participation (Matthews and Prothro, *Negroes and the New Southern Politics*, p. 308). In both cases, the expression of white hostility is a response to black mobilization, not a cause of it.

Because this finding seems to fly in the face of so much social science research into the causes of lower-class protest, however, it may give rise to the suspicion that the measures of discrimination used here are faulty. Some reflection on the dynamics of southern rural society may dissipate these suspicions and make sense of these findings. Most students of protest movements acknowledge that deprivation has to be perceived to be politically relevant. In the case of rural blacks in Mississippi, however, the variations in degree of discrimination among counties are simply too small to notice, and they pale in comparison to the absolute differences between white and black income and education levels *within* each county. Moreover, the relevant reference groups for Mississippi blacks since the advent of radio and television include northern whites and blacks in addition to blacks in other Mississippi counties. Compared to these northern reference groups, Mississippi blacks must feel so deprived that they can hardly perceive any difference between their own condition and that of blacks in other Mississippi counties. For all of these reasons, it is not very surprising to discover that differences among Mississippi counties in the amount of black relative deprivation have no noticeable effects on black political behavior. Like the variations in hostility, the variations in discrimination against blacks among Mississippi counties are too insignificant to have a political impact.

Apathy and Black Political Participation

That variations in black vulnerability, and not variations in white hostility or discrimination, seem to account for variations in local black political participation still does not resolve all the doubts surrounding our explanation of black participation in terms of *fear*, however. The possibility still exists that what we have identified as measures of *vulnerability* may really be measures of something else entirely. Common sense, moreover, lends credence to such a possibility: it seems reasonable, after all, to expect that the degree of independence of a black community would correspond rather closely to its level of education, income, and social status—the three factors normally identified with the dominant "apathy model" of political participation. Such a finding would not necessarily discredit the view that fear, and not these other variables, was really the *causal* factor. After all, a correlation or regression coefficient is only a measure of statistical relationship and cannot establish which of two interrelated factors is the cause. Nevertheless, the case for the contention that fear of economic harassment and intimidation is the real cause of low black political participation in Mississippi would clearly be stronger if it were possible to demonstrate that income, education, and social status—the three factors that comprise the "apathy model" of participation—were *not* also significantly related to black participation.

To test these notions, five different measures of income, four different measures of education, and one measure of social status were employed. The alternative measures of income and education, all derived from the 1960 *Census of Population,* are listed below:

Income

NW* Median Family Income
NW Median Individual Income
Percentage of NW families with Incomes Under $1000
Percentage of NW families with Incomes Under $2000
Percentage of NW families with Incomes Over $4000

Education

Percentage of NW Population with No School
Percentage of NW Population with 0–4 Years School
Percentage of NW Population with Some College

*NW = nonwhite

Each of these measures permits us to focus on a slightly different dimension of the phenomenon. For example, the "percentage of nonwhite families with incomes less than $2,000" measures the poverty population, while the "percentage of nonwhite families with incomes greater than $4,000" measures the size of the black middle class.

Social status was measured by means of the Socio-Economic Status Index, a standard sociological measure combining measures of income, education, and occupation into a single index by means of a simple weighing formula developed by the Population Division of the U. S. Census Bureau. This index thus has the virtue of providing a standardized measure of status, and therefore permits us to determine how relevant standard notions of status are to the particular situation of blacks in Mississippi.[43]

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[42] Addition of the income discrimination variable to the "expanded fear model" boosts the explained variation by only 1.5 per cent, and the likelihood that any chance factor could have produced the same result was an unacceptable 30 per cent. Addition of the education discrimination factor contributes only 2.3 per cent to the explained variation, but with a probability of 20 per cent that the same result could have been produced by chance.

Income and Participation. Of all the "apathy-related" correlates of political participation, income is probably the most important. In his comprehensive study of political participation in *Political Life,* for example, Robert Lane concluded that income affects voting turnout even more than does education.[44] In addition, the impact of income variations on black turnout is particularly important because of the implications it may have for the real meaning behind the "fear model." If economic independence, a prime component of the "fear model," turns out to be only a surrogate measure of income level, and if the level of income of a county's black population turns out to be a good predictor of its participation rate, then the whole interpretation of the dynamics at work presented above may be thrown into question.

In fact, however, neither of these relationships holds. In the first place, there is no significant correlation between a black community's income level and its degree of economic *dependence* as measured by occupation ($r = -.200$). Not surprisingly, therefore, there is no significant correlation between the income level of a county's black population and its rate of political participation—no matter which measure of income we use. (See Table 7.) Evidently, poverty is *not* the real cause of black nonparticipation in politics: fear is, and fear born of economic peonage. The *source* of income, not simply the *amount,* is the crucial determinant of political liberty, as we suspected.

Table 7. Correlations Between Five Measures of Income and Black Political Participation in Twenty-Nine Mississippi Counties

Measure of Income	Correlation Coefficient
Nonwhite (NW) Median Family Income	+.011 (n.s.)
NW Median Individual Income	+.274 (n.s.)
Per cent of NW Families with Income Less than $1000	+.055 (n.s.)
Per cent of NW Families with Income Less than $2000	−.191 (n.s.)
Per cent of NW Families with Income Greater than $4000	+.152 (n.s.)

Table 8. Correlations Between Four Measures of Black Education and Black Political Participation in Twenty-Nine Mississippi Counties

Measures of Education	Correlation Coefficient
Per cent of NW Population with No School	−.331
Per cent of NW Population with 0-4 Years School	−.414*
Per cent of NW Population with Some College	+.332
NW Median Education Level	+.401*

* Significant at the .05 level.

Education and Participation. Unlike variations in black income levels, variations in black educational levels among Mississippi counties do seem to have a significant impact on black participation rates. In particular, as Table 8 above suggests, black voter turnout tends to be highest in counties with relatively high educational levels, and lowest in counties with low educational levels. What is more, because a county's black educational level turns out to be closely related to the degree of black economic vulnerability,[45] this finding raises the possibility that the effect we have ascribed so far to economic invulnerability should really be ascribed to educational attainment.

Whether this finding discredits the view that fear, and not apathy, really accounts for variations in black political participation rates among Mississippi's 29 black majority counties, however, depends on the interpretation attached to these educational variations. A correlation, after all, is simply a measure of relationship and cannot by itself "explain" the relationship. Rather, explanation can come only from careful analysis of the dynamics of the particular social situation.

In most of the literature on political participation, to be sure, the link between education and participation is explained in terms of interest and understanding, both of them aspects of apathy. Campbell, Converse, et al., for example, argue that educated people participate in politics more than the uneducated people do because the educated command more "facts about politics" and have more sophisticated concepts with which to "maintain a sense of order and meaning amid the flood of information. . . ."[46]

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[43] The nonwhite SES Index for Mississippi counties was taken from: Mississippi State College, "Socio-Economic Status Indexes for Mississippi Counties," by Ellen S. Bryant, *Bulletin 724* (State College, Mississippi: Mississippi State University Agricultural Experiment Station, April 1966).

[44] As Lane points out: "Perhaps, for a simple conventional act such as voting, income is more important, while more complex forms of participation are more dependent upon qualities associated with education." Robert Lane, *Political Life,* p. 222.

[45] The correlation between nonwhite median education and the percentage of a county's labor force in Vulnerable Occupations is −.773, an extremely high figure.

In the context of Mississippi society, however, this interpretation of the social meaning of the education figures is open to serious question. In the first place, it is questionable whether the number of years that blacks spend in local public schools has any relation at all to the amount of politically relevant knowledge they have acquired. Mississippi black public schools are notoriously bad places to learn about politics, for discussion of politics has long been *verboten* in these citadels of conservatism. In the study of the reasons for the failure of black schoolteachers to register to vote in Mississippi cited earlier, for example, Prothro discovered that even social science teachers neglected to discuss politics in class for fear of losing their jobs.[47]

In the second place, while the public schools failed as providers of politically relevant information, other sources filled the gap; but these sources were available to the educated and the uneducated alike. Television, which is now widely available in rural Mississippi, permitted even the poorly educated to learn their constitutional rights from the lips of Dr. Martin Luther King at Montgomery, and Selma, and Washington, D.C. The evening news on radio and television provided nightly lessons in political participation that were unavailable in the classroom. To assume, in short, that formal education signifies greater political comprehension may be to misread the facts. But how, then, are we to explain the clear statistical link between the level of nonwhite formal education and the rate of black political participation?

One answer, it seems, lies in an alternative explanation of the meaning of education in Mississippi's "closed society."[48] Rather than measuring the amount of understanding of politics, formal education for blacks in Mississippi may in reality be another measure of "sense of security." The relevant social fact buried in the education statistics may really be the level of literacy in a county, not the level of political sophistication and understanding. And literacy is more properly an index of vulnerability than of comprehension. A person who can read may know little more about politics than one who cannot in rural Mississippi, but he probably has some confidence that his ability to read will save him from at least some dangers. The illiterate lacks even this meager sense of security. Feeling more vulnerable as a result, he naturally experiences greater fear. What is more, the illiterate voter does not even enjoy the "secret ballot" in elections. To vote, he must enlist the assistance of a polling official (who is virtually always a local white) and must vote aloud. In an already frightening situation, the prospect of voting out loud for the Movement's candidates must deter many illiterate blacks from voting, or else induce them to vote against the black candidates.[49] The observed correlation between education and participation, therefore, may really reinforce, not contradict, the argument that fear is the cause of nonparticipation.

This conclusion finds support, moreover, in the data reported in Table 8 above. The measure of educational attainment that was most powerfully related to black participation was none other than the one measuring *the extent of illiteracy* in each county (namely, the percentage of blacks more than 25 years old with 4 years or less of education). Perhaps then, education in these counties affects participation through the mechanism of fear, not through the mechanism of political comprehension and apathy. Further support for this conclusion can be found in the fact that adding the education factor to the factors already measuring vulnerability in the "expanded fear model" produces only a marginal addition to the amount of variation in black participation rates accounted for.

Social Status and Participation. The only other major apathy-related factor that could be hiding in the data on black dependence and vulnerability is "social status." Numerous scholars have discovered that higher-status individuals participate in politics more regularly than do those of lower status, and have explained this phenomenon in the now familiar terms of apathy: higher-status individuals have greater self-esteem, a greater sense of personal efficacy, and therefore a greater interest in politics. If what we have earlier taken to be measures of dependence and vulnerability turn out to be surrogates for measures of social status, the whole argument developed above would again be thrown into question.

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[46] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter: An Abridgment* (New York: John Wiley and Sons, Inc., 1964), p. 251.

[47] U. S. Commission on Civil Rights, *Hearings*, Jackson, Mississippi, 1965, I, 211–12.

[48] The term "closed society" was used by historian James Silver to characterize the lack of political liberty in Mississippi in the late 1950s and early 1960s. See: James W. Silver, *Mississippi: The Closed Society* (New York: Harcourt, Brace, and World, Inc., 1963), especially p. xvi.

[49] For an example of how these pressures worked, see: Salamon, "Mississippi Post-Mortem," pp. 45–7.

At first glance, the data seem to confirm this impression. The correlation between nonwhite socioeconomic status indexes and black voter turnout was a significant +.440, suggesting that the social status of a county's black population is a fairly good predictor of its political participation rate. The problem, however, is that much of this result reflects the influence of the *education* component of the SES index, not the income or occupational components.

To separate out these influences, the three components of the "apathy model" were tested together using multiple regression analysis to assess the explanatory power of the model as a whole. The results, reported in Table 9 below, indicate that even if we interpret income, education, and social status the way exponents of the "apathy" theory of political participation suggest, "apathy" can still account for only 23 per cent of the variation in black participation rates, compared to the 69 per cent accounted for by "fear."

Conclusions and Implications

Of the three possible explanations of variations in black political fortunes in Mississippi's 29 black majority counties formulated earlier, therefore, the explanation based on "fear" is clearly the one that finds most support in the evidence, as Table 10 indicates. Taken together, the four measures of vulnerability embraced in the "expanded fear model" account for 69 per cent of the variation in black political participation rates, with a probability approaching zero that this result could have occurred by chance. The nearest competitor is the "apathy model," but it explains only 23 per cent of the observed variation, and this result could have happened purely by chance more than 5 per cent of the time, rendering this model statistically insignificant. While these findings do not formally "prove" that fear is the real explanation of black voting patterns, they certainly create a strong presumptive case for this conclusion.

Table 9. Test of the "Apathy Model": Impact of Three Apathy-Related Factors on Black Political Participation in Twenty-Nine Mississippi Counties

Factor	Percentage of Variation in Black Participation Accounted for by this Factor *and All Above It* (Multiple R^2)
Median NW Education	16.0%*
Median Family Income	16.1
Socioeconomic Status Index	23.4

* Significant at the .05 level.

Table 10. Comparison of Three Models of Black Political Participation in Twenty-Nine Mississippi Counties

Model	Percentage of Variation in Black Participation Rates Accounted For (Multiple R^2)
"Expanded Fear Model"	68.6%**
"Discrimination Model"	10.3
"Apathy Model"	23.4

** Significant at the .01 level.

The evidence presented here thus squarely challenges the hopeful assumption embodied in the 1965 Voting Rights Act, the assumption that the elimination of the formal, legal barriers to black voting in the Deep South would, by itself, unleash latent "forces of democracy" presumably lying dormant in the southern hinterlands. This assumption has been sustained over the past half-decade only by seizing on every black political victory as proof of its validity while ignoring the bleak record of defeat or attributing it to black apathy or contentment. By contrast, the data analyzed here call for a more thorough rethinking of the preconditions for democracy in the Deep South. If economic dependence and fear of intimidation frustrate political participation for blacks in the Deep South, as the evidence here suggests, then democracy can not be achieved simply by formal legal manipulations of the sort embodied in the Voting Rights Act, important as those manipulations are as a first step. Nor, significantly, will it be achieved simply by raising black living standards. Income, we found, was a singularly poor predictor of black political mobilization. So long as increases in income do not liberate blacks from direct economic dependence on local powers-that-be, and thus leave blacks vulnerable to intimidation or fear of intimidation (as has been the case, for example, with the teachers), improvements in income per se will have little impact on political participation. In other words, not the *amount* of income, but the *source* is the crucial factor in determining participation.

The same is true, moreover, with regard to education. Education, we suggested, affects participation mainly to the extent that it relieves feelings of insecurity and vulnerability. But its ability to do so is severely circumscribed by the social and economic relationships in the society at large, and particularly by the persistent fear of coercion carried over from the past, and reinforced frequently enough in the present to survive into the foreseeable future.

The pursuit of democracy in the Deep South, therefore, must acknowledge the continued impact of economic vulnerability and fear on political action. Democracy will emerge only to the extent that these conditions—and not just the formal, legal barriers—are eliminated.

This conclusion not only has analytical implications, however; it has policy implications as well. In particular, in our view, it throws new light on the potential importance of a truly nationally administered welfare program such as the Nixon Administration's original Family Assistance Plan hinted at. Whatever its specifically economic impact, the analysis presented here suggests that such a scheme could have an even more important *political* impact. By taking control of welfare payments away from local powers-that-be, it could liberate a sizable army of "welfare serfs" from the fear of losing their sole source of income if they support black candidates for political office. No single program under serious consideration today, we believe, could protect so large a segment of southern blacks from the fear of economic intimidation and thus open the way for expanded black political participation.[50]

The findings presented here may have broader implications as well, however, for the Voting Rights Act of 1965 falls squarely into the same tradition as the one that has guided American foreign policy in the "developing nations" for decades. With its stress on the politico-legal barriers to democracy in the Deep South, the Voting Rights Act reflects what Robert Tucker has termed America's "instrumentalist bias," its preoccupation with the outward trappings of democratic government in the "developing societies" and its neglect of the substantive underpinnings of democracy.[51] Americans, political theorist Louis Hartz has noted, characteristically ignore the "deeper social struggles" under way in the developing nations and concentrate instead on the more pragmatic question of procedure. "We fail," Hartz notes, "to appreciate *non-political definitions of freedom* and hence are baffled by their use."[52]

By demonstrating the limitations of this politico-legal approach to democracy even in the Deep South, where it receives constant sustenance from dominant national norms and plentiful national economic resources, the findings presented here thus raise even more serious questions about its prospects abroad. These findings suggest that the accepted wisdom among political scientists about the causes of nonparticipation in politics may be seriously in error, at least as it applies to disadvantaged populations in politically underdeveloped areas. Apathy, it seems, has carried too large a part of the explanatory burden for too long. It is time to acknowledge as well the role of fear.

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[50] For further discussion of this point, see: Lester M. Salamon, "Family Assistance: The Stakes in the Rural South," *The New Republic* (February 20, 1971), pp. 17–18; and Stephen Van Evera, "Welfare Reform: Bonanza for Dixie's Blacks," *Progressive*, June 1972, p. 27. Recent versions of this idea, by eliminating the provision for national administration, threaten to destroy the potential liberating impact of the plan.

[51] Robert C. Tucker, "Russia, the West, and World Order," in Robert Tucker, *The Soviet Political Mind* (New York: Praeger Publishers, Inc., 1963), pp. 181–82.

[52] Louis Hartz, *The Liberal Tradition in America*, A Harvest Book (New York: Harcourt, Brace and World, Inc., 1955), p. 306.

Comment: A Re-evaluation of Black Voting in Mississippi*

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Lester Salamon and Steve Van Evera have undertaken a difficult task. To choose among competing explanations of individual political behavior which for the most part make similar predictions—as do their "apathy" and "fear" models—should be difficult enough, but to do so with aggregate, county level data would seem nearly impossible. Yet the authors make the effort and are rewarded with some unexpected and interesting findings. Most surprising and central to their "fear model" of black voting in Mississippi is the discovery that indices of economic vulnerability (or dependence), and not poverty or education per se, most closely relate to black voter turnout. On the basis of this finding they recommend a federally administered welfare program such as President Nixon's Family Assistance Plan. Not only would it increase the number of beneficiaries and raise the level of income for many impoverished blacks, but more importantly it would liberate them from the coercion of "welfare serfdom,"—as the authors put it—at the hands of local white welfare administrators. A second unexpected finding, and it seems to me underappreciated by the authors, is the strong positive relationship between the percentage of the voting age population that is black and black voter turnout. This discovery is especially curious and noteworthy since earlier research (to be described below) agrees in reporting a *negative* relationship between the percentage of blacks in a community and the level of black voting registration.[1]

Before embracing the authors' policy recommendations or revising conventional wisdom on the subject, several questions should be considered about their analysis and conclusions:

(1) Is the fear model adequately discrete? In its "simple" form, the fear model posits that economic vulnerability and not socioeconomic status best explains nonvoting. At this stage the model presents a test which may either confirm or deny its hypothesis, and may, therefore, be useful. When it is "expanded," however, to include standard organizational explanations of participation, serious problems surface. For one, the model no longer provides a test to demonstrate that fear is operating. Although the authors provide a fascinating and plausible account of how organization may increase black participation by displacing fear, organization may also operate to "displace" apathy resulting from generations of poverty, poor schooling, and a heritage of nonparticipation enforced by law. Even without systematic economic intimidation extensive political organization would be necessary to stimulate mass political participation among Mississippi's black citizenry. American history is replete with instances of political oranizations successfully enfranchising and mobilizing large and otherwise nonparticipant segments of the population for whom fear as economic coercion was not a primary restraint from participation.[2] The point is that "fear" is not necessary to our understanding why organized counties record higher levels of black voting than unorganized counties.

A second problem with the "expanded fear model" is that it is too versatile. For example, with respect to the important variable, percentage of the county's voting age population that is black, fear can account for either a positive or negative relationship with black participation. The authors find a positive correlation and argue that percentage black is an important organizational resource and as such helps to displace fear. The earlier studies mentioned above, however, found a negative correlation and proffered an explanation equally congenial to the authors' fear model. Whites threatened by

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[1] Donald R. Matthews and James W. Prothro, *Negroes and New Southern Politics* (New York: Harcourt, Brace and World, 1966), pp. 115–120. Donald S. Strong, "The Future of the Negro Voter in the South," *Journal of Negro Education*, 26 (1957), 400–407. Johnnie Daniel, "Negro Political Behavior and Community Political and Socioeconomic Structural Factors," *Social Forces*, 47 (March, 1969), 274–280. A weaker relationship between these two variables is reported by John H. Fenton and Kenneth N. Vines in "Negro Registration in Louisiana," *American Political Science Review*, 51 (1957), 704–713.

[2] The most notable example is the growth and operation of urban political machines during the 19th and early 20th centuries.

black influence and control in local elections more thoroughly exercise social and economic sanctions against participation in communities where blacks constitute a large portion of the population.[3] Thus, as the percentage of the voting age population that is black increases, voting becomes a more dramatic and forboding act and vulnerability becomes a powerful inducement to stay home. Stated and tested simply then, either finding can be rather easily incorporated into their model. Later in the paper, I shall suggest a framework for reconciling the earlier research with the authors' present finding. But in its current condition the "expanded fear model" subsumes relationships which may have little to do with fear and permits *post hoc* confirmation by opposite findings.

(2) Are the findings accurate? The dependent variable, black voter turnout, is measured during the 1968 presidential election, while the data for independent variables are obtained from the 1960 census, some nine years earlier. During the 1960s many Mississippi counties experienced considerable change in the relative size and demographic features of their black populations. For example, by the 1970 census five of the twenty-nine counties with black population majorities in the 1960 census no longer had black majorities. Moreover, there were substantial changes in their collective social and economic characteristics. During these ten years the percentage of the black population with a high school education doubled—despite the fact that outward migration appears to be very high among young, educated adults.[4] The percentage of the black population with a yearly income under 2,000 dollars declined from two-thirds in 1960 to less than one-third in 1970, and the percentage with less than a 4,000 dollar yearly income dropped from ninety-one to sixty per cent. Perhaps the most dramatic changes occurred in agriculture. With widespread mechanization, tenant farming and sharecropping became an uneconomical system of agricultural production, and, as Table 1 shows, tenancy experienced a precipitous decline during the 1960s. What was at one time the primary means of livelihood for blacks, today has virtually disappeared in many counties. There is also a slight but steady decline in the number of black farmowners.

Table 1. Drop in the Number of Black Mississippi Tenant Farmers during the 1960s

	1959	1964	1969
Number of Black Farmers	54,927	37,715	17,184
Number of Tenants	32,243	18,580	2,657
Percentage of Farmers Who Are Tenants	58.7	49.3	15.4

Source: *1969 U.S. Census of Agriculture, Mississippi* (Part 33, Section 1, Volume 1) Table 3, p. 3.

Although for many counties throughout the nation 1960 census data may adequately approximate the characteristics of its 1968 population, in Mississippi when looking at the black population which is undergoing rapid social change this may not be the case. Thus, any judgment of the relative effects of income levels, education, racial composition, and farm tenancy (a primary component of Salamon and Van Evera's economic vulnerability index) on black political participation must be tentative until supported by the more accurate data available in the 1970 census.

(3) Are the findings generalizable? Salamon and Van Evera begin by asking why black candidates have failed to win many elections in counties where they appear to constitute a voting majority. This naturally led to an analysis of twenty-nine counties which had black population majorities (although not all of these, even by 1960 figures, actually had voting age majorities). Their findings support certain hypothesized causes of low participation in the important black belt counties of Mississippi, yet they may be wholly inappropriate for explaining participation in other countries with smaller proportions of black population. Because the proportion black in a community (the dimension on which the twenty-nine counties were selected for examination) may itself be related to the independent and dependent variables, we cannot simply assume that the reported relationships are pervasive throughout the state. Given the provocative nature of the findings, the authors are naturally prompted to state general conclusions and make policy recommendations. Before this becomes acceptable, however, the general applicability of their findings must be ascertained.

With these questions in mind, the analysis will be replicated with some important modifications. First, 1970 census data will be used to measure the independent variables. As indicated above, there is reason to suspect that the figures for the various social and economic indices are significantly different from those reported in the 1960 census. (For a description of index construction and intercorrelations among selected independent variables see Appendix A.) Second, the investigation will be expanded to include all eighty-two of Mississippi's counties, providing an opportunity to test the reported findings in counties with varying racial composition. In order to do this the dependent variable, black voter turnout, must be operationally redefined. The previous study averaged the Humphrey vote and the vote for local black candidates (when available) to arrive at a "mobilization index."[4] In this analysis only the Humphrey vote will be employed, since in 1968 only a relatively small number of counties had black candidates running for office.[5] Salamon and Van Evera report that the Humphrey vote correlates with the vote for the local black candidate at .92; therefore, the difference in the two indices should have little effect on the results. (For a discussion of the validity of using the Humphrey vote as a measure of turnout for the other Mississippi counties, see Appendix B.) Third, the stability of the findings over time will be checked by examining black turnout during the 1971 election in which Charles Evers, the black Mayor of Fayette, Mississippi, ran an unsuccessful statewide campaign for governor against the regular Democratic nominee, William Waller.[6] By using more accurate census figures, expanding the scope of the study, and extending it over time we obtain a richer and more reliable set of data for examining the effects of fear, apathy, and politics on black political participation.

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[3] Matthews and Prothro, 115–117.

[4] John P. Thames, "Population Migration Effects on Mississippi Poverty," *Mississippi Law Journal,* 39 (May, 1968), 423–450.

Replicating the Analysis

Three principal explanations of variations in black turnout have been described in the previous paper. They are apathy, presented as the conventional social science model and measured by the standard socioeconomic measures; fear as measured by occupational vulnerability; and political organization which in this analysis can only be indirectly indexed by percentage black of the voting age population. The last two were combined to form the "expanded fear model" of participation. Because of the objections to this merger raised earlier, however, each explanation will be evaluated as separate and distinct. This does not, of course, preclude the possibility that two or indeed all three explanations may be operating simultaneously and in a complementary manner.

In Table 2 the multiple correlations for the variables central to the several explanations are displayed.[7] In some respects the results are similar to those discovered by Salamon and Van Evera. The indices of occupational vulnerability consistently correlate with black voting participation at about the same levels as the joint correlation of median education and family income. The important point here is that the coefficients for the vulnerability variables reported in Table 2 are not nearly so large as those reported by Salamon and Van Evera in their Table 3. It is curious that 1960 data would produce stronger correlations with voter

Table 2. Correlations of Selected Socioeconomic Variables with Black Voting Participation in Mississippi

	1968		1971	
	All Counties	29 Selected Counties	All Counties	29 Selected Counties
	(Multiple Correlation Coefficients)			
Vulnerability Indices				
% Vulnerable % Invulnerable, Revised	.12	.25	.37	.42
Socioeconomic Indices				
Med. Education Med. Family Income	.26	.27	.44	.59
% of Voting Age Population, Black	−.04	.41	.19	.40

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[5] The county level voting returns for the 1968 presidential election are taken from F. Glenn Abney, compiler, *Mississippi Election Statistics* (University, Miss.: Bureau of Governmental Research, 1968), supplement, no pages given.

[6] The general election was held in November, 1971. Charles Evers, running as an Independent, as did many other black candidates for local office, received 23 per cent of the total vote. County level returns are taken from . . . *In the Public Interest* (Millsaps College: Institute of Politics in Mississippi) Vol. 2 (June, 1972), 3.

[7] The reader is cautioned to remember that the relationships are for ecological variables and that the danger of unjustifiably inferring individual-level associations from correlation coefficients is great. Because individual cases have not been grouped (into counties) according to their scores on the dependent variables (black voter turnout in 1968 and 1971) unstandardized regression coefficients should be more revealing about individual level "effects." Where opportune, regression slopes based on b scores will be used. For a lucid discussion on this point see W. Phillips Shively. "'Ecological' Inference: The Use of Aggregate Data to Study Individuals," *American Political Science Review*, 63 (December, 1969), 1183–1196. See also Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: Univ. of North Carolina Press, 1964), chapter 4.

turnout in 1968 than the more accurate, updated figures. Some of the difference could conceivably lie in the slightly modified indices (refer to Appendix A and Table 2 of the preceding paper). A more probable reason for such a pronounced discrepancy, however, is that vulnerability as measured in 1960 compared with 1970 represents essentially a different variable. The size of the tenant population given in Table 1 heavily contributes to the total percentage of the labor force in dependent occupations in 1960, especially in the black belt counties of the Delta, the last stronghold of the southern-style plantation system. (Almost all of the Delta counties are included among the authors' twenty-nine predominantly black counties.) Reflecting the massive decline of farm tenancy recorded by the 1969 agricultural census, this occupational category no longer contributes so heavily to the percentage of dependent workers. By late 1968 the 1960 census figures of the percentage in vulnerable occupations in a county may reveal less about the individual features of the county's black labor force than it does about a general rural profile of the county. Perhaps it is this contextual element which adds to the explanatory power of Salamon and Van Evera's measure.[8]

As with Salamon and Van Evera's finding, the percentage of the voting age population that is black (percentage V.A.P., Black) closely correlates with participation in the twenty-nine counties with a large black population for both the 1968 and 1971 elections. If the same causal relationship exists throughout the state, then the statewide correlations should be even larger, given the increased variation. Table 2, however, reveals no relationship at the statewide level in 1968 and only a weak one for the 1971 election. This finding suggests that the percentage V.A.P., Black should be more closely scrutinized within ranges of black-white population ratios; in areas where there is a small black proportion this variable may operate differently.

All that the multiple correlations for the vulnerability and socioeconomic indices in Table 2 can do is to suggest that the two sets of variables are equally powerful in explaining variation among counties in black voter turnout. They tell us nothing about the relative merits of the two explanations. For this, partial relationships are needed. In Table 3 all five variables covering the three explanations are included in a multiple regression analysis. The coefficients are beta's, standardized regression coefficients similar in this case to fifth order partial correlation coefficients.[9] Assuming that these variables are causally independent, we can compare the relative contribution of each to black turnout. Given the small N upon which the computations are based, the reader is advised not to pay too much attention to the absolute value of the coefficients or to small differences among them. Instead the table is intended to display which variables are "operating" under controls and which are not. Clearly the vulnerability indices, with only one exception (per cent vulnerable in 1971, for all counties), fail to display sizable relationships under controls.[10] Thus, fear as

Table 3. Partial Regression Coefficients (Beta's) for All Five Independent Variables

Election Year	Data Base (counties)	% Vulnerable	% Invulnerable, Revised	Median Fam. Inc.	Median Education	% VAP, Black	Total Variance Explained (r^2)
1968	All	−.04	−.07	−.24	.51	.12	8.3%
	29	−.05	.13	.28	.13	.60	36.5%
1971	All	−.33	.16	−.44	.51	.45	38.2%
	29	.02	.04	−.17	.68	.48	54.6%

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[8] For a brief review of the literature and discussion of the employment of ecological data to measure contextual or structural variables see Tapani Valkonen, "Individual and Structural Effects in Ecological Research" in *Quantitative Analysis in the Social Sciences,* ed. Mattei Dogan and Stein Rokkan (Cambridge, Mass.: The M.I.T. Press, 1969), pp. 53–68.

[9] Hubert M. Blalock, Jr. describes this as the "quantitative" criterion for evaluating the relative importance of independent variables in "Evaluating the Relative Importance of Variables," *The American Sociological Review,* 26 (December, 1961), 866–874.

[10] Salamon and Van Evera use percentage of black families who own their homes as a check on the occupational dependence variables but subsequently drop this index from their analysis. Including percentage of black homeownership with the other two vulnerability indices slightly increases the total multiple correlation coefficient. Upon close inspection the per cent of black homeownership was found largely to duplicate the effects of the occupational measures, and therefore it will not be employed in this analysis. Housing data were taken from the *1970 Census of Housing* (part 26: Mississippi [Washington: G.P.O., 1971]).

economic intimidation is seriously weakened as the primary model of black voting. Median family income does retain some correlation, but in three of four instances the direction switches from positive to negative. Although this does not necessarily mean that *among individuals* increasing income will produce nonvoting it does suggest what Salamon and Van Evera concluded from their findings, that the impoverished condition of the black community does not have any independent depressant effects on county-to-county variations in black turnout.[11]

Education emerges from the regression analysis as the single most important independent variable. With the exception of the twenty-nine heavily black counties in the 1968 election, median educational level of black adults has a pronounced independent effect on turnout. This finding is certainly consonant with a massive amount of research which shows that for individuals education is closely tied with various forms of political participation. Salamon and Van Evera also found that median education is a powerful predictor of voting, and after a cursory analysis of the correlations of various levels of education with turnout (see their Table 8) concluded that education reflects fear more than apathy. They argue that since the percentage of the black population with little or no schooling correlates more closely with turnout ($r = -.41$) than does the percentage with some college ($r = .33$), then illiteracy must be the main reason for nonparticipation. Because illiterates often require assistance in voting, usually from white election officials and poll watchers, they are more susceptible to intimidation.[12] Undoubtedly such incidents of intimidation have occurred and do occur, but with the data available it is impossible to determine whether illiteracy primarily reflects fear or apathy. We can only speculate on the cause. Even with fear absent we should expect to find illiteracy highly correlated with nonparticipation. Yet the authors conclude, "Education, we found, affects participation mainly to the extent that it relieves feelings of insecurity and vulnerability." The manner by which education affects voting among blacks in Mississippi is more problematic than Salamon and Van Evera acknowledge.

In Table 5 of Appendix A, the percentage of the voting age population that is black is shown to correlate strongly with the main independent variables representing the fear and apathy models. Thus, the possibility exists that the effects of percentage V.A.P. Black are spurious and will be eliminated with the introduction of appropriate controls. The regression coefficients in Table 3 refute this hypothesis completely. In each instance the percentage V.A.P. Black increases in its relative importance over its zero-order relationship, and for three of the four regressions, it is among the most powerful variables present. *To conclude, median education and the percentage of the voting age population that is black emerge under controls as the most influential variables in producing county-to-county variations in black turnout.*

A Closer Look at the Racial Composition of the Electorate

Black voting is rather curiously related to the proportion black of a county's voting age population. At the statewide level weak zero-order correlations between these two variables are shown in Table 2, yet for the twenty-nine heavily black counties, there is a strong positive relationship between percentage V.A.P. Black and turnout during both elections, even under multiple controls. Apparently the effects of this variable on turnout are shaped by the racial composition of the sampled counties. In Figure 1 this suspicion is confirmed. For both elections the relationship is similarly curvilinear and nonmonotonic.

The means of percentage voting in Figure 1 plot the strong positive relationships first identified in Table 2 between percentage V.A.P. Black and turnout in the heavily black counties. A closer examination of the means across the range of percentage black reveals that this association occurs only after the black proportion of the voting age population reaches the sixty per cent level. This finding fits neatly into Salamon and Van Evera's discussion of this variable as an organizational resource. *Only in settings where black candidates have a good chance of winning do increases in proportion black have a positive effect on black turnout.* Apparently when the potential black majority is between 50 and 60 per cent the practical chances are too slim to stimulate much activity, and below 50 per cent the likelihood of winning is remote indeed.

In counties where the black citizenry constitutes only a small proportion of the total electorate an inverse relationship appears; among counties with less than 30 per cent of the eligible voters black, the correlations between percentage V.A.P., Black and turnout are

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[11] Median family income is closely related to community poverty levels, correlating $-.93$ with percentage of black families listed in poverty by the 1970 census, and .88 with the percentage having less than a 2,000 dollar yearly income.

[12] Recent cases of white election officials either misdirecting or not assisting black illiterate voters is documented in *The Shameful Blight* prepared by the Washington Research Project (Washington, October 1972), pp. 82–87.

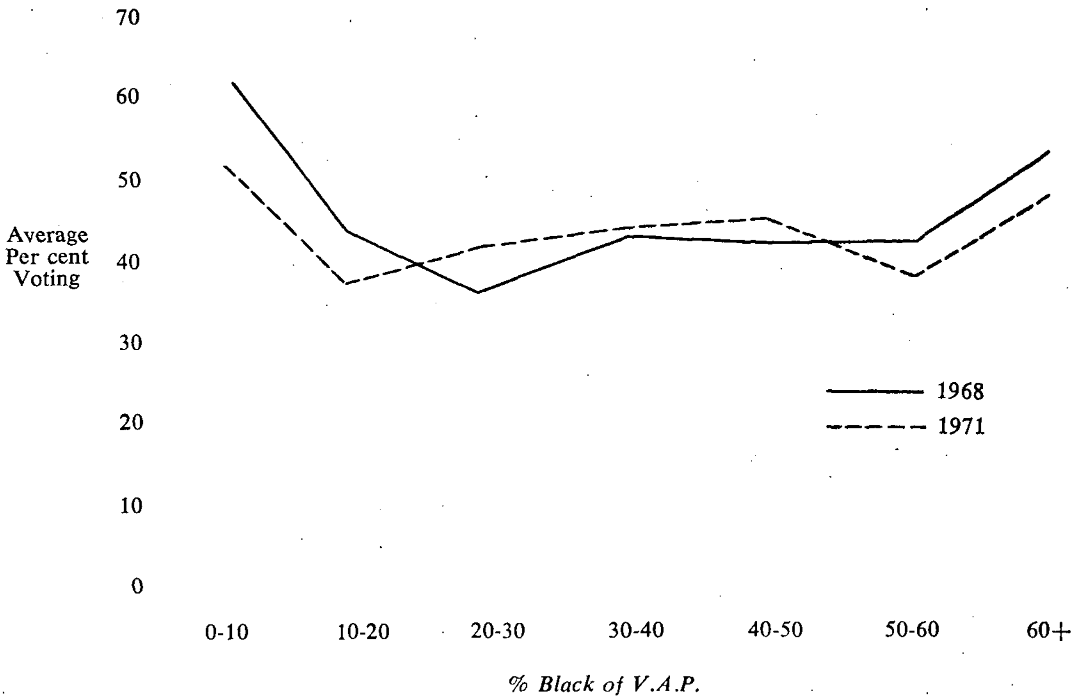

Figure 1. The relative size of the Black electorate is related in a curvilinear and nonmonotonic manner with Black voting turnout.

—.56 for 1968 and with only two cases in the lowest population range in 1971 a positive .09. Among counties in the intermediate range of 30 to 44 per cent black,[13] increases in the percentage V.A.P. Black appear to produce little change in voting turnout with correlations at .00 and —.06 for the 1968 and 1971 elections, respectively.

Thus far, a reason has been offered for the upturn in participation at the 60 per cent level of Black V.A.P., but none has been suggested for the inverse relationship in 1968 at the lower ranges. In some respects these counties display a pattern similar to that reported by earlier studies in which black registration throughout the Deep South decreased as the black proportion of the county's population increased. Donald Matthews and James Prothro in their definitive study on black participation prior to the civil rights movement find that the rate of decline in percentage registered accelerated as the percentage black of the county's population approached majority status, presumably reflecting stricter social control on the part of the white community as black enfranchisement threatened white dominance in local politics.[14] In contrast, Figure 1 depicts a decelerating rate of decline which levels off in the 20–40 per cent range. Thus, whether the decline in turnout for 1968 reflects decreasing white tolerance or some other phenomenon is questionable. It may be that threat perception among whites begins at a lower threshold in Mississippi than in the other southern states. We know that before the civil rights movement, black registration in Mississippi was by far the lowest in the South, hovering around the three per cent mark.[15] Perhaps, because of the increasing social and legal constraints on some forms of intimidation and the unavailability of various methods of disenfranchisement common before the civil rights movement, black participation on the average cannot be lowered much below the forty per cent figure. Thus, participation drops sharply to this plateau and then flattens out. Also to be considered is the likelihood that as the proportion black increases, black voters will constitute voting majorities for certain subcounty offices. Because of highly segregated residential patterns there are many "beats" which contain black majorities even though the county as a whole has a white electoral majority. Black candidates have since 1968 done fairly well in elections for such subcounty offices as constable and justice of the peace. Perhaps this electoral situation which naturally occurs with greater frequency as the proportion black increases operates to increase voting and offsets to some extent pressures against participation. Admittedly this argument is highly speculative and *ad hoc,* but it does serve to identify additional variables that may be important to participation but cannot be easily tapped in a study of this nature.[16]

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[13] Forty-five rather than 50 per cent is used as the cut off for several reasons: (1) Because of differential outward migration some of these counties which in 1970 had less than a black voting age majority, may well have had a black majority in 1968. (2) In 1968 with the most recent census nine years old ambiguity probably existed as to the actual size of the potential black electorate and thus the same incentives to organize politically may have operated. (3) Since a 50 per cent cutoff produces only minor and inconsequential changes in the slopes given in Figure 1, and the 45 per cent provides a more desirable distribution of cases, the lower percentage figure is used here and in subsequent figures.

[14] Matthews and Prothro, *Negroes and New Southern Politics*, Figure 5–1, p. 116.

[15] Ibid., p. 151.

In sum, what is being suggested is that the relationship between participation and percentage black may be a modified version of the relationship found before the civil rights movement. As a result of federal laws and voting rights activity, the potential black electorate in heavily black counties has been liberated and has become an incentive and a resource for political organization. In the other counties where blacks do not constitute large voting majorities, the effects of the voting rights activities have been less pronounced, and the more traditional pattern of a negative relationship emerges, although even among these counties there has been some impact with mean turnout registering above the 30 per cent figure. By this reasoning, *the effects of the civil rights movement have been greatest in counties where the black citizenry constitutes a potentially large electoral majority.* To this point the argument has been somewhat conjectural, tailored to fit the curves in Figure 1. We can do more, however, than merely debate its merits; there exists other evidence which can be brought to bear on its adequacy.

Some Mississippi counties experienced greater civil rights activity than others. For example, prior to passage of the major voting rights legislation some counties witnessed extensive civil rights campaigns from outside organizations such as, the NAACP, the Voter Education Project, and in 1964 the umbrella organization COFO, while others remained apparently untouched.[17] And subsequently, although all Mississippi counties were required by the Voting Rights Act of 1965 to end *de jure* disenfranchisement, by 1968 less than half received federal registrars and overseers.[18] In comparing counties which experienced greater and lesser degrees of civil rights activity, the explanation presented above makes several important predictions: (1) Counties with potential black majorities which experienced the civil rights movement should display a substantially larger turnout than the overall average. (2) Counties in which this movement was largely *absent* should follow the pre–civil rights pattern of a negative relationship, with the turnout being *lowest* among counties with a large percentage of the V.A.P. Black. (3) Therefore, exposure to voting rights activity should produce its largest differences (effects) among counties with potential electoral majorities.

Without in depth, county-by-county information it is impossible fully to assess the degree to which the civil rights movement "entered" and affected a given county. If we assume, however, that the presence of federal registrars in a county represents an adequate surrogate measure for more comprehensive but unavailable data on civil rights activity we can further test the conclusion made above that the voting rights activity had its greatest effects in heavily black populated counties.[19]

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[16] There is some chance that this early negative slope may to some degree be an artifact of the composition of the counties' populations. In counties with very small black populations an even small white defection may greatly affect the percentage of black turnout, since the number of whites who defect may represent a relatively large percentage of the potential black electorate. Thus, the mean turnout percentage for the counties at the extreme left end of Figure 1 may be less accurate than for counties with larger black populations.

[17] Pat Watters and Reese Cleghorn, *Climbing Jacob's Ladder* (New York: Harcourt, Brace and World, 1967).

[18] The counties which had received federal examiners by late 1967 are identified in *Political Participation* compiled by the United States Commission on Civil Rights (Washington: G.P.O., 1968), Table 9, pp. 244–247.

[19] There is good reason to believe that the presence of registrars is an important ingredient in black political mobilization. For example, by 1968, in Mississippi counties with federal examiners present, 71 per cent of the black eligible voters were registered as compared to only 50 per cent registered in non-examiner counties. Harrell R. Rodgers, Jr. and Charles S. Bullock, III, *Law and Social Change* (New York: McGraw-Hill Book Co., 1972), p. 32. Close observers of the civil rights movement in Mississippi have often asserted that the Child Development Group of Mississippi, the umbrella agency sponsoring Head Start projects in twenty-nine counties, created in its citizen advisory councils the core of political leadership in these counties. This variable was found to add little explanatory power to the effects of federal registrars, however.

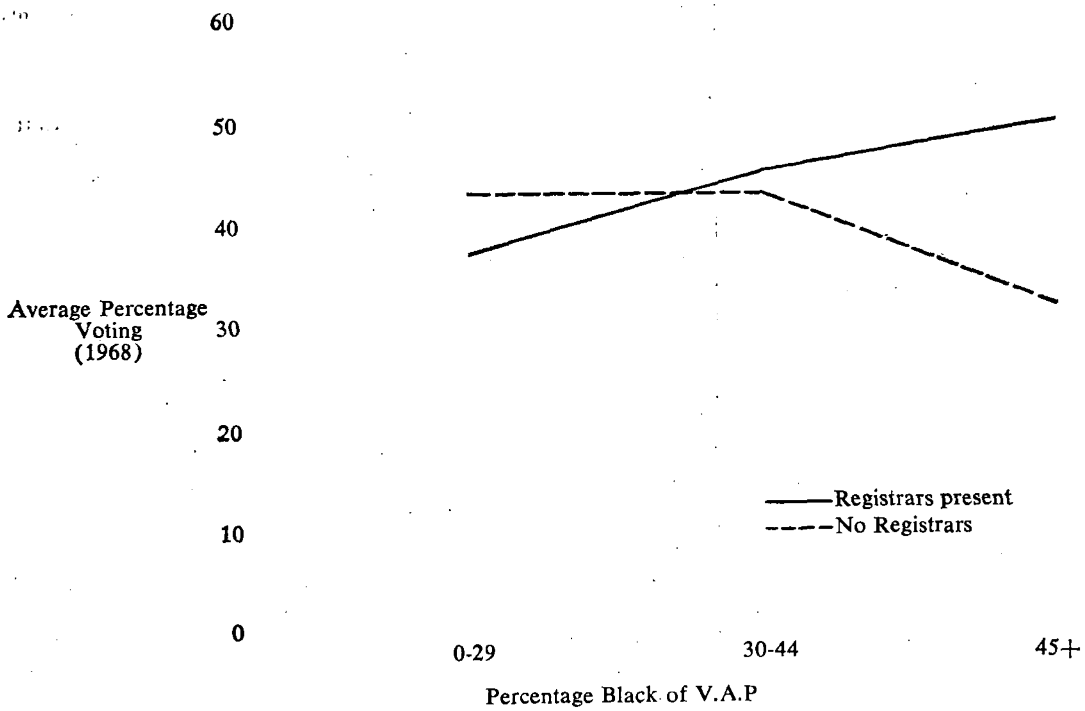

Figure 2. The Direction of the Relationship between Black Voting Participation and Per cent of the V.A.P. that is Black is Conditioned by the Presence or Absence of Federal Registrars.

In Figure 2 we can compare voter turnout in 1968 for counties with and without federal registrars. Just as predicted, voting turnout is highest for counties where the black citizenry constitutes a voting age majority and federal registrars were present. The second prediction is also confirmed; in counties less affected by the civil rights movement (i.e., no federal registrars) the relationship between percentage V.A.P. Black and voting closely corresponds with the pre–civil rights movement pattern reported by Matthews and Prothro among others. In these counties as the percentage V.A.P. Black increases, the per cent voting decreases, especially as the proportion black reaches a potential electoral majority. (It should be noted that although the pattern is similar the range of variation in turnout is much smaller.) Thus the differences between counties with and without federal registrars present are greatest among those counties where the black V.A.P. is in the majority. As speculated above, *one of the most important consequences of the civil rights movement appears to be the mobilization of black electoral majorities.*

From the viewpoint of assessing the civil rights movement in the Deep South, we can conclude that private and governmental efforts during the mid-1960s had their greatest impact in communities where the return should be greatest whether measured in terms of sheer numbers of new voters or in the acquisition of political power. Of course, if only a small number of predominantly black counties were affected by the movement, then although the impact on them may be great, its overall significance for the extension of political equality would be minor. We can see from Table 4 that this is not the case. In fact, federal registrars went disproportionately into the predominantly black counties and as a result optimally concentrated their energies and resources. Moreover, indications are that during the early stages of the movement when the burden was carried primarily by private groups the focus was similarly

Table 4. Relation Between Presence of Federal Registrars and Percentage V.A.P. Black

		Registrars Present in County	
		Yes	No
Percentage V.A.P. Black in County	Under 45	20	42
	45+	11	9

$N=82$
$r=.40$ (based on continuous percentaging)

on the black majority counties.[20] *Judged by a cost-benefit perspective, the civil rights movement fares' well; resources and attention were focused on areas which yielded the highest return.*

Why Black Candidates Fail to Win Elections

For those of us who value the expansion of voting rights the preceding paragraph reports good news. Yet although the civil rights movement appears to have effectuated greatest change in communities containing a sizable black electorate with some chance of electoral success, expectations of frequent black victories in county elections are not being fulfilled. Even when the black electorate contains a large majority the elections are usually close, and often the black candidate loses. Why is this so?

First, black voting certainly needs to expand further. Realizing that black voting early in the 1960s was virtually nonexistent, one finds it hard to gainsay the successes of the civil rights movement. Both in 1968 and 1971 forty-two per cent of the eligible black voters were voting, and according to the most conservative estimate about three-quarters of those registered voted in 1971.[21] Mass black political participation is a reality; in 1971 more than 170,000 voted. Given the dire social and economic conditions of this subpopulation, it may be expecting too much to look for a substantially higher turnout in the near future. Many black citizens have yet to enter the system as participants, and until more do so the chances for victory in marginal counties are small.

A second important variable which affects the chances of a black victory is so obvious that it is generally ignored; it is the white vote. Perhaps white turnout is intuitively viewed as a constant and for this reason is overlooked. *White voter turnout, however, largely represents a response to the potential and actual black turnout.* White voting correlates with percentage V.A.P. Black .42 in 1968 and .46 in 1971, and it correlates with the black turnout .10 and .40 for the 1968 and 1971 elections respectively. The slopes of these relationships, which are plotted in Figure 3, represent the effect of the given independent variable on white turnout, the steeper the slope the greater the effect. It is interesting that in 1968 and 1971 the independent contribution of percentage V.A.P. Black is the same. This variable represents in a sense the potential threat of black voting to white control. As the relative size of the black electorate (percentage V.A.P. Black) increases, so does white turnout. This strongly suggests an important reason why black candidates frequently lose elections they should win. In counties where black candidates have some chance of winning, white voters are going to the polls at a very high rate and often an otherwise decent black showing is insufficient.

The second variable, the actual percentage of blacks voting probably reflects the mobilization of black voters in registration drives and election campaigns and as an index of this mobilization correlates highly with white turnout. Its increased impact on white turnout (compare the regression slopes of Figure 3A with 3B and the partial coefficients in 3C) during the 1971 election may have been due to the presence of a black candidate running for the state's highest office. Charles Evers began his gubernatorial campaign as an attempt to create a coattail to assist black candidates running for county and state legislative offices. The possibility of actually winning was initially acknowledged as unlikely. As election day neared, however, the campaign changed. Evers began soliciting hitherto nonexistent white liberal support as well as the vote of newly enfranchised youth. Purchasing local television time, he began to appeal for support from a larger constituency which may have diverted attention from getting out the black vote for local candidates.[22] When the vote was in, his candidacy was rewarded with only 23 per cent of the total votes cast and spot checks of a number of all white precincts show almost total opposition. The extent to which his campaigns simply increased the white turnout against him and other black candidates can only be guessed, but given the evidence that black voter mobilization has the side-effect of also increasing the white turnout, the adverse consequence may have been large. More generally, this finding suggests that black mobilization in Mississippi should proceed as quietly and inconspicuously as possible. Any effort to appeal for white support will probably just make the black candidacy more salient to whites and result in a net loss of votes.[23]

Summary and Conclusion

The central thesis of Salamon and Van Evera's ambitious effort is that fear as measured

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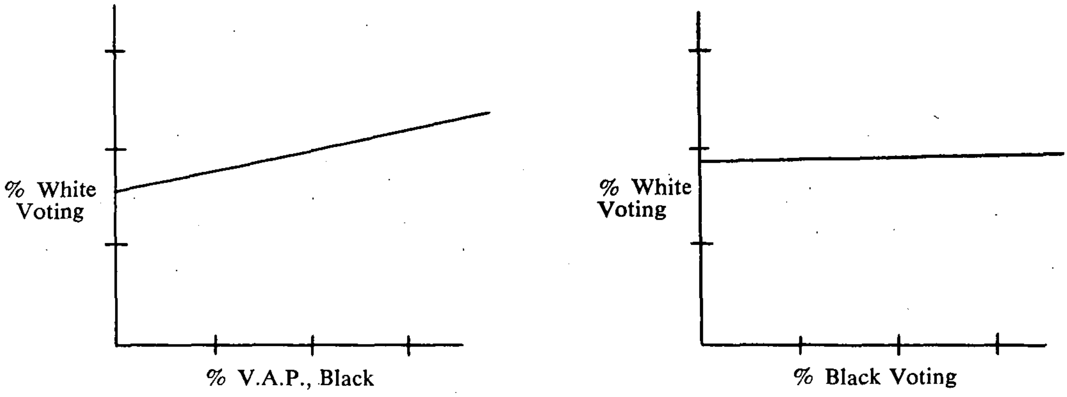
[20] Watters and Cleghorn.

[21] . . . *In the Public Interest*, p. 3.

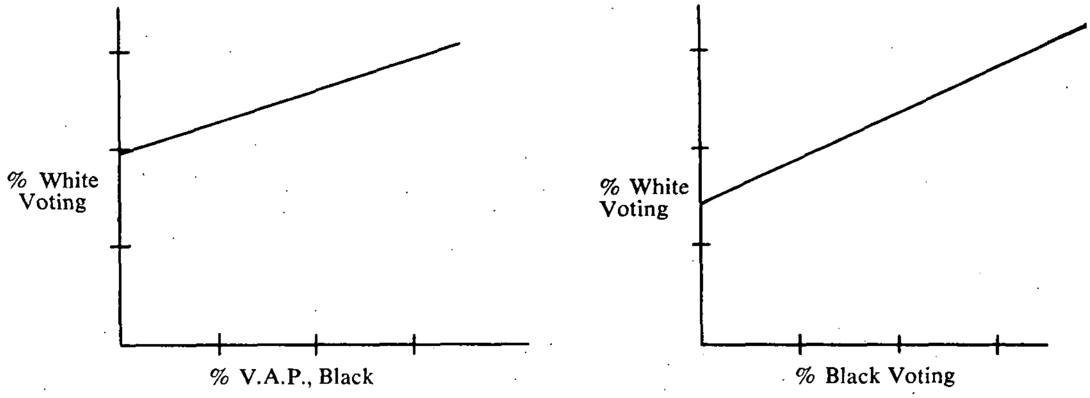
[22] . . . *In the Public Interest*, pp. 4, 6.

[23] Fred Wirt in his important study of integration in Panola County, Mississippi, reports that this consideration is appreciated by candidates of both races. "A candidate active among the other race stood to gain very few votes but more likely would mobilize even more voters against him because of the adverse racial reaction." (*Politics of Southern Equality* [Chicago: Aldine, 1970], p. 161).

A. 1968 Election



B. 1971 Election



C. Partial regression coefficients (b's).

	% V.A.P., Black	*% Black Voting*	*Explained Variance (r^2)*
1968	.28	.06	20.7%
1971	.26	.39	30.1%

Figure 3. White Voter Turnout Reflects the Level of Black Voter Mobilization.

by occupational vulnerability is the primary explanation of black voting. On re-examination of this explanation using an expanded data base, fear as measured does not appear to have much independent importance in explaining county-to-county variations in black turnout. This is not to say that at the individual level there are no effects, but simply that the ecological data fail to suggest it. Education, on the other hand, is found in both studies to be a powerful and stable factor in black political participation. Whether education primarily reflects the level of vulnerability to intimidation and manipulation or instead represents the standard political interest and awareness concomitants so frequently found for other populations, can with the evidence presented here only be argued. Surely individual cases of both fear and apathy resulting from poor education could be found. It is persuasive support for the standard "apathy" model, however, that the level of college education among the blacks in a county is almost as important as the county's level of black illiteracy in explaining black voting turnout.

Both studies have discovered that among heavily black populated counties, variations in the percentage black of the voting age population is an important factor in black turnout. This strong positive relationship was present for both the 1968 presidential and the 1971 gubernatorial elections. Salamon and Van Evera and I agree that this variable is probably indicative of the level of organizational activity on the part of aspiring candidates as well as continuing political groups within a county, but whether these organizations operate primarily to displace "fear" or "apathy" remains in doubt. By expanding the analysis to include counties with smaller black populations we find that the percentage black of the electorate changes dramatically in its relationship with turnout. At the intermediate population range (30 to 44 per cent black of V.A.P.) there is virtually no relationship with voting turnout, and among counties with a comparatively small black population the relationship actually changes direction. Additional evidence suggests that the conversion from a strongly negative to a strongly positive relationship among counties with potential black voting majorities may be one of the chief successes of the voting rights campaigns and federal voting laws of the 1960s.

Winning elections and sharing political power is not simply the result of getting out the black vote. First, in counties with a potentially large black electorate, white turnout normally is high. Second, as black organizations mobilize their own constituency with registration drives and election campaigns, they unintentionally mobilize the white vote in opposition. Consequently, as their turnout increases, the threshold vote necessary for winning is also raised.

Salamon and Van Evera and I agree that black voting in Mississippi is in some sense subject to peculiar influences. Despite our apparent disagreement over the relative merits of certain explanations of black voting, we concur in paying close attention to contextual factors not commonly found in voting research, the most important being the size of the potential black electorate. Moreover, the study of black political participation in the Deep South produces a unique confluence of several fields of inquiry in political science. Research and theory in such diverse areas as voting behavior, law and social change, and political development can all inform and be informed by the study of black voting in this region. Thus, research into black political participation represents an opportunity not only to describe and understand the process of mass enfranchisement of the last major population group in the country, but also may provide in its uniqueness a rich field for testing the breadth and adequacy of theories of political participation and political development.

Appendix A. Index Construction

All of the social and economic data have been taken from three sources: *General Population Characteristics: Mississippi* (no. 26, 1971); *General Social and Economic Characteristics: Mississippi* (no. 26, 1972), *1969 Census of Agriculture: Vol. 1 Area Reports* (section 2, County Data, part 33: Mississippi).

Occupational Vulnerability. This index of economic dependency and vulnerability to white economic coercion closely follows Salamon and Van Evera's index of "most vulnerable" occupations given in Table 2 of their paper. This index is composed of the percentage of the county's black labor force (age 16 and over) which is either unemployed or employed as farm tenants, household workers, farm laborers and foremen; although unmentioned in their Table 2 it must be assumed that foremen were included in their count since the 1960 census also lists laborers and foremen under a single heading.[24]

A revised index was constructed by adding to the above occupations all black salaried agricultural workers and local governmental employees (including school teachers). Comprising a somewhat larger proportion of the black labor force, this new measure correlates with the original index at .91. Since the two measures are so highly correlated, the revised occupational vulnerability will be dropped from the analysis, and only the measure employed by Salamon and Van Evera will be retained.

Occupational Invulnerability, Revised. It was not possible nor desirable to duplicate completely the authors' "least vulnerable" category. The number of workers employed outside their county of residence—included in their index—is not indicated in the 1970 census reports. Also, in the 1960 census, as the authors acknowledge, the "professionals" category included highly vulnerable school teachers. Comprising the "least vulnerable" or "invulnerable" occupations are farm owners, nonlocal government workers, nonagriculture self-employed, mining, and manufacturing.

Median Education. Unfortunately the census reports median education for the sexes separately. After an inspection of statistical relationships between median education for each sex and the dependent variables confirmed that

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[24] Salamon and Van Evera note that schoolteachers are highly vulnerable, but because the 1960 census included teachers in the category of "professionals" they could not be included among the highly vulnerable occupations. The 1970 census which lists teachers separately permits their inclusion among the highly vulnerable. The presence of this relatively small group, however, had almost no effect on the relationships.

		(1)	(2)	(3)	(4)	(5)
% Vulnerable	(1)		—.59	—.74	—.41	.16
% Invulnerable	(2)	—.43		.73	.25	—.33
Median School	(3)	—.73	.23		.50	—.25
Median Family Inc.	(4)	—.55	.02	.76		—.37
% VAP(21+) Black	(5)	.51	—.24	—.66	—.61	

Table 5. Intercorrelations Among Selected Independent Variables.

(Correlations to the left of the diagonal are for all counties (N = 82); the coefficients to the right of the diagonal are for the 29 counties in the Salamon-Van Evera paper.)

male and female median education have virtually identical "effects" on turnout, they were combined (averaged) to create a single, although slightly imperfect, median education index.

Percentage of the Voting Age Population that is Black (% V.A.P. Black). For the 1968 election the voting age population is based on the percentage aged 21 and over. Several weeks before the registration deadline for the 1971 general election, however, voting rights were extended to 18- through 20 year olds. Although few of the newly eligible voters managed to register in time, they are included in the calculation.[25] Using the 21-year-old voting age population index instead produced minor changes in the correlations with the 1971 turnout.

Black Voter Turnout. For 1968 this variable was formed by dividing the total Humphrey vote in a county by the black voting age population. For 1971 the Evers vote is employed, and the eligible electorate included ages 18–20.

Appendix B. Validity of the Humphrey Vote as a Measure of Black Turnout

The Salamon and Van Evera paper reports an extraordinarily high correlation (r = .92) between the Humphrey vote in 1968 and the vote for local black candidates. Averaging the two measures is redundant, and as the authors note using either index alone yields the same results (see footnote 21). This indicates that the Humphrey vote alone suffices as a valid index of black voting. Of course, the same caveats issued by the authors hold here. In a few instances, to be sure, black voters voted against Humphrey and against the local black candidate either from mistake, fear, or choice, but as a measure of county-to-county variations in black voting the Humphrey vote remains satisfactory.

Some readers may question the validity of this measure for the other fifty-three counties with a smaller black population. Perhaps in these counties there is less polarization between the races and consequently more crossover voting. Intuitively this seems unlikely. More plausible is the hypothesis that the same dynamics of voting preferences are operating equally in all counties. Moreover, the evidence supports the latter viewpoint. Below are correlated county figures for the Humphrey and Evers vote (in 1971). The Humphrey vote correlates closely with the vote for the black gubernatorial candidate three years later.

Correlations between:

Total for Humphrey and Total of Evers, r = .98.

% of Total for Humphrey and % of Total for Evers, r = .94.

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[25] . . . *In the Public Interest* (Millsaps College: Institute of Politics in Mississippi), Vol. 1 (August, 1971), 1–14.

Fear Revisited: Rejoinder to "Comment" by Sam Kernell

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AND

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In his interesting comment on our article, Sam Kernell raises several searching objections to our analysis that throw some doubt on our central conclusion about the role of fear in black participation in the South; but then proceeds to demonstrate the usefulness of our analysis by extending its application over both time and space and confirming several of our hypotheses in the process. While we are somewhat jealous that Mr. Kernell did not have the same opportunity we had to have his results rendered slightly out of date by this *Review's* lengthy publication delays, we are nevertheless grateful to him for the support his research provides to several of our earlier findings and for the opportunity it affords us to revisit our hypotheses in the light of more recent elections and Census data. Despite the thoughtfulness of his criticisms, however, we believe that many of his objections to our analysis are misdirected and that his effort to replicate our analysis is seriously incomplete and hence misleading. To demonstrate this, we propose to consider each of his objections to our original article in turn, specify why his purported replication of our analysis falls short of a true replication, and finally, present the results of a true re-testing of our earlier hypotheses against more recent data. What we discover from this three-pronged effort is that our earlier conclusion—that fear has been a major determinant of black political participation in the Mississippi "black belt"—survives both the scrutiny Mr. Kernell ably gives it and the updating to which we subject it ourselves. In the process, moreover, we are able to demonstrate the apparent utility of the models we developed earlier in measuring an important dimension of political change, a dimension we will refer to as the transition from "fear-based nonparticipation" to "apathy-based nonparticipation," and that we believe could be generally useful in examining the "opening" of political systems to real popular participation in the course of political modernization.

Evaluation of Objections

Kernell raises seven major objections to the analysis presented in our article. While each of these objections has some merit, each also has several shortcomings and must therefore be scrutinized with care. Kernell's first objection, for example, is that it is difficult to choose among our competing "fear" and "apathy" explanations of political participation because they "make similar predictions" (p. 1307). In fact, however, as Table 10 in our article indicates, the "fear model" accounts for 69 per cent of the variance in our dependent variable, black turnout, while the "apathy model" accounts for only 23 per cent. How these can be construed as making "similar predictions" is difficult to understand.

Kernell's second objection, also offered in his opening paragraph, is a contention that hypothesis-testing using aggregate analysis is "nearly impossible." Not only does this fly in the face of the recent methodological discussions defending the use of ecological techniques cited in our article (footnote 15), but also it raises the question of why Kernell then proceeded to use aggregate analysis himself. We are aware of the shortcomings of ecological research and took pains both to warn our readers and to minimize the dangers. In addition, we underlined the dangers that survey research also embodies for the purpose we had in mind. More recently, we have examined what survey data is available to determine whether the relationships we discovered at the aggregate level can be verified as well at the individual level. As we will illustrate below, the results are supportive.

A third objection to our analysis raised by Kernell deals with the *discreteness* of our fear model. In particular, Kernell questions the inclusion of our organization variables in the fear model on grounds that organization may affect black voter participation through some mechanism other than fear. Which explanation of the dynamics really at work behind a set of numbers can never be determined from the numbers themselves, of course. We therefore agree with Kernell that readers should look beyond the numbers into the dynamics of the social situation to interpret the true meaning of any empirical indicator such as our organization variables; and we took pains in the body of the article to emphasize this very point and to explain our interpretation of this set of variables. Despite Kernell's hypothetical objections, we still find this explanation convincing. The important point to re-emphasize here is that our decision to include this set of variables in our fear model was not based on mere conjecture or *post-hoc* rationalization. To the contrary, it was based on extensive interviews with numerous civil rights activists in the South, on a review of written accounts of civil rights organizing in the Deep South (see footnotes 5 and 34 in our text), and on a detailed analysis of black organizational development in three Mississippi counties performed by one of the present authors (LMS).[1] To help guarantee we were tapping the fear-related aspects of organization, moreover, we chose empirical indices that promised to focus in on this particular dimension. This was particularly true of our "outside organizer" and "per cent of VAP, black" variables. The whole logic behind interpreting the "per cent VAP, black" as an organizational variable is that the risks associated with voting become bearable only as the prospects of winning become better. We are gratified, therefore, that Mr. Kernell himself ultimately concludes that his own finding "fits neatly into Salamon and Van Evera's discussion of this variable [the black proportion of the voting age population] as an organizational resource" (p. 1311). In addition, we find the alternative argument about the role of organization—an argument that rests on the analogy between black-belt civil rights organizations and big-city political machines—unconvincing, if only because the civil rights organizations have had no access to the social services and patronage that constituted the real basis of the urban machines' mobilizing ability. To attribute the same dynamic as the "machine" to a set of embryonic political organizations that completely lacked the major resources available to the machine—services and patronage—is, we think, inappropriate. For all of these reasons, we believe that our interpretation of the meaning of civil rights organization in the southern black-belt is valid and our decision to include this set of variables in the fear model consequently appropriate.

Kernell's fourth complaint about our "expanded fear model" is that it is "too versatile." In particular, he suggests that "with respect to the important variable, per cent of the county's voting age population that is black," fear can account not only for the positive relationship with black participation we found but also for negative relationships because, as earlier writers have suggested, white hostility increases as the real potential of black power increases, and hence blacks should participate less as the black proportion of the population rises. However, Kernell's observation here conveniently overlooks two crucial parts of our analysis that were aimed explicitly and precisely at this point, and that we believe kept us well out of the pitfall he claims we succumbed to. First, we examined *only* the black majority counties largely to eliminate the effect on black participation of variations in white hostility (which is really what Key and Matthews and Prothro were measuring with their per cent black variable, as Kernell concedes). By focusing only on counties where the potential of black electoral victory was real and where white hostility was consequently relatively high, we deliberately avoided the charge of "versatility" Kernell nevertheless levels at us by holding constant the white hostility impacts of variations in black population proportions, and measuring only the impact that variations in the black proportion of the population above this minimum had on black organizational potential. No content with this safeguard, however, we employed a second one to determine whether choosing counties in this fashion really produced the control we intended. In particular, we formulated an explicit set of white hostility measures and tested them against the participation data to guarantee that the results we were attributing to variations in black vulnerability were not really attributable to variations in white hostility. The results, reported in Table 5 of our article, supported our assumption. Yet Kernell overlooks this analysis. In view of this, his statement that our model "permits *post hoc* confirmation by opposite findings" seems incorrect to us.

A fifth objection raised to our analysis by Mr. Kernell is that our findings are not accurate because, as Mr. Kernell puts it, "the dependent variable . . . is measured during the 1968 presidential election, while the data for the independent variables are obtained from the 1960 census, some nine years earlier." Although Mr. Kernell is certainly correct in noting that we did not use the 1970 census (because it was not available until two years after we completed our analysis and 18 months after we submitted our article to the *APSR*), we disagree strongly with his conclusion that this renders our results inaccurate—for several important reasons. First, contrary to what Mr. Kernell claims, our independent and dependent variables are not all separated by nine years. For example, the basic index computed from local elections held largely in *1967,* and the 1968 presidential election was used primarily as a check on the local election figures. As for the independent variables, they were certainly not all "obtained from the 1960 census," as Kernell claims. For example, the "organizer index," one of the four components of our "expanded fear model," was obtained from interview sources in 1969 and covered the entire period of the early and mid-1960s. More importantly, the very independent variables of greatest concern to Kernell, those relating to agriculture, were not taken from the 1960 Census at all, but from the Agricultural Census of *1964*. What makes these observations especially important, moreover, is the fact that the dramatic social and economic changes that Kernell claims might invalidate our results did not occur at a uniform rate throughout the 1960s. To the contrary, the really massive agricultural dislocations cited by Kernell were concentrated in the latter part of the decade, after the extension of the federal minimum wage law to agricultural laborers in 1966 and the switch from the commodity distribution relief program to the food stamp program in 1967.[2] At the time of the 1967 local elections, and even the 1968 presidential elections, therefore, these changes were still very much in progress. Since social and economic changes of the sort that occurred in Mississippi beginning in 1966 do not take place overnight, we believe the Census of Agriculture figures for 1964, the Census of Population figures for 1960 and our organizer index for 1961–1968 come closer to depicting reality at the time of the 1967 local elections and the 1968 presidential election than do the comparable 1970 figures; for by 1970 the changes that were only beginning to be felt massively in 1967 were already far more complete. Interestingly, moreover, Kernell's own findings, reported in his Table 2, lend credence to this view, for almost every one of his 1970 independent variables does a better job of accounting for the 1971 black turnout (as measured by the Evers vote) than it does in accounting for the 1968 black turnout (as measured by the Humphrey vote).

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[1] This analysis will be published shortly by Indiana University Press as part of Lester M. Salamon's book-length study of social and political change in Mississippi and the American South.

A final problem with this objection Kernell raises about the accuracy of our analysis is that it overlooks the most important aspect of the dislocations that occurred between 1960 and 1970 and hence implies a far greater change in results from using the 1970 figures instead of the earlier ones than is in fact the case. In particular, Kernell focuses his attention exclusively on *absolute changes within counties.* What is most relevant for correlation and multiple regression analysis, however, are *relative changes among counties.* Only if the changes that occurred affected counties differently will they influence the results. However, Kernell neglected to compute the correlations between the 1960 or 1964 values of the key variables and the 1970 values. Table 1 illustrates the results of such a computation and suggests the inadequacy of Kernell's focus on absolute instead of relative changes. For example, while Kernell, in the absence of systematic examination, argues that "vulnerability as measured in 1960 compared with 1970 represents essentially a different variable" (p. 1310), the correlation between these two for the 29 black majority counties we examined turns out to be a high +.772. In fact, except for the median family income measure (the weakest of the variables), the 1960/64 values of all the key variables are *at least* this closely related to their 1970 values. By focusing on gross, absolute changes and ignoring the persistence of more stable variations among counties, Kernell thus further weakens his contention that our analysis is inaccurate. Despite Kernell's criticism, therefore, we feel justified in believing that our original independent variables are not only valid, but preferable, for examining the 1967 and 1968 elections. Moreover, the generally high correlations between the 1960/64 values of these variables and the 1969/70 values leads us to suspect that the same analytical model that explained the 1967/68 results will continue to

Table 1. Correlations Between 1960/64 Values of Key Variables and 1970 Values

Variable	Correlation Coefficient
% Vulnerable	.772
N. W. Voting Age Population	.992
N. W. Median Education	.905
N. W. Median Income	.488
% Voting Age Population, Black	.880

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[2] The extension of the minimum wage law gave planters in the black-belt counties a powerful incentive to shift to mechanized agriculture and dismiss black field laborers, while the switch to food stamps completed the social squeeze process by requiring cash payments of relief recipients who had formerly been able to stay alive during the workless winter months by securing free commodities. For a discussion of the consequences during 1967 and 1968, see: Tony Dunbar, *Our Land Too* (New York: Random House, 1972).

have substantial, if somewhat diminished, explanatory power when redefined using 1970 data and applied to the 1971 elections. The reanalysis to be presented below generally confirms this suspicion and, in the process, suggests an interesting new way to measure an important aspect of "political development."

Kernell's sixth objection to our analysis is a challenge to the *generalizability* of our findings, particularly their generalizability to white majority counties in the South. We readily concede this point. We made no claim that our results would be appropriate for "explaining participation in other counties with smaller proportions of black population" (Kernell, p. 1308). Indeed, our decision to focus on the black majority counties reflected this very supposition. Our theory was that interpersonal coercion would tend to affect participation most significantly only where the stakes in an election are high, *and* the outcome of the election is in doubt and could be reversed by coercion. While the first of these conditions holds in virtually all Mississippi counties, the second applies only to those with black voting age population majorities or near majorities. By extending our analysis to all Mississippi counties, therefore, Mr. Kernell has importantly complemented our research by demonstrating what we would have predicted: that our findings about the role of fear do *not* seem generalizable to the white-majority counties in Mississippi. However, we do not agree with Mr. Kernell that this means our findings are not generalizable at all. For while our explanation of participation patterns in the Mississippi black-belt may not apply to the white majority areas of Mississippi, we believe it may apply quite well to other parts of the world—to those places where social conflict is intense, where electoral institutions are called upon to handle this conflict, where the outcome of elections is in doubt, and where those in power have the means physically or economically to coerce those who are seeking power. Several portions of the so-called "developing world" probably meet these specifications. Indeed, as we suggested in our original article, the presence of a "fear-based pattern of participation" may be viewed as an important defining characteristic of an "underdeveloped" or "premodern" polity. To the extent this is true, our analysis could provide a fruitful basis for cross-national empirical research that might yield quite interesting generalizations about political participation patterns in developmental settings. Our analysis, in short, may be far more generalizable than Kernell contemplates.

Kernell's final major objection to our analysis deals with our effort to interpret the social reality denoted by the relatively high correlation between black participation rates and black education levels. We have no quarrel with Kernell's major point that "with the data available it is impossible to determine whether illiteracy primarily signifies fear or apathy." Indeed, we took pains in the article to emphasize this very point. What we were eager to suggest, however, was that the conventional explanation of the meaning of education in terms of apathy was not the *only* explanation possible; that an explanation that interpreted education in terms of fear was at least as plausible, and possibly more so in the peculiar circumstances of the southern black-belt. In questioning this suggestion, however, we believe Mr. Kernell inaccurately characterized the chain of reasoning that led us to this alternative explanation. In his view, our interpretation grew directly out of "a cursory analysis of the correlations of various levels of education with turnout," as if a set of numbers were sufficient to generate a theory. In fact, our chain of reasoning was the exact reverse of what Mr. Kernell suggests: we developed our alternative explanation on the basis of what we have learned about the dynamics at work in this society and only then turned to the data to determine what empirical credence it gave to our theory. Finding some, we cited it for what it was worth, warning readers as we went that "a correlation . . . is simply a measure of relationship and cannot by itself 'explain' the relationship." (p. 1303). Readers must choose for themselves which interpretation of the education variable seems most plausible. For our part, we continue to believe that a large portion of the relationship between education and turnout in the southern black-belt can be attributed to education's relationship to fear. Even so, we conceded the point to adherents of the conventional interpretation and took the conservative course of excluding the education variable from our expanded fear model. In retrospect, and despite Kernell's objection, we believe a strong case could be made for reversing this decision and incorporating education into our fear model.

Replication Problems

Against the background of his objections to our analysis—objections we generally find unconvincing for the reasons just cited—Mr. Kernell proceeded to undertake what he terms a replication of our analysis using more recent data for the independent variables and extending the analysis in time and space to embrace the 1971 elections and all Mississippi counties. In large part, Kernell's findings mirror our own. Yet they also partially challenge the conclusion we reach about the importance of fear in explaining black turnout. In evaluating this challenge, however, it is important to understand certain crucial respects in which Mr. Kernell's analysis differs from ours.

Although we naturally concur that 1970 figures are appropriate for analyzing the 1971 election, we have already explained why we think our own earlier figures are preferable in analyzing the 1967–68 elections. Even more important than this difference in data base, however, are additional disparities that differentiate his approach from ours even when we use the same 1970 data base and focus on the same 1971 election in the same 29 counties. In a word, Mr. Kernell offers only a partial replication of our original analysis, yet neglects to note this partiality. In the first place, he completely ignores one of our three models, the "discrimination model." In its place he substitutes what he calls the "political organization model," which is really a fragment of our "expanded fear model." Even here, however, Mr. Kernell is not true to our original analysis, for two of the three organizational variables included in our "expanded fear model"—the black voting age population and our organizer index—mysteriously disappear from his analysis completely, without so much as a mention. The conclusion Kernell draws from his Table 2 that the fear and apathy models are "equally powerful" in explaining black turnout rates thus seems somewhat misleading, since the "apathy model" as evaluated in this table still contains both its income and education components, whereas two of the original four variables in the "fear model" have been dropped from the analysis altogether and a third broken off and set up as an autonomous explanation. For these and other reasons, therefore, we feel that Mr. Kernell's purported replication fails to do justice to our original analysis.[3]

Fear and Apathy Revisited—Measuring Political Change

To correct the shortcomings in Mr. Kernell's purported replication of our original analysis

Table 2. Regression Equations for "Expanded Fear" and "Apathy" Models of Black Voter Turnout, 1967/8 vs. 1971, Twenty-Nine Mississippi Counties

Model	Variables	Beta (Standardized Regression Coefficients)		% of Variance Explained (R²) Cumulative	
		1967/8	1971	1967/8	1971
"Expanded Fear"	% Vulnerable	−.59	−.42	28.9	13.9
	N. W. Voting Age Population	−.37	−.51	29.5	25.7
	% VAP, Black	.26	.34	46.5	44.8
	Organizer Index	.46	.28	68.6	49.6
"Apathy"	SES Index	.71	N.A.	7.3	N.A.
	Median Education	−.21	.76	23.3	33.4
	Median Income	−.32	−.35	23.4	42.6

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[3] In addition to the differences already cited, Mr. Kernell proceeds differently in defining the vulnerability index. While we used the black voting age population as the denominator in computing this index, Kernell uses the black "labor force," which subjects his index to the vagaries inherent in the Census definition of the labor force and different cultural patterns among households. The labor force, after all, includes not all adults, but only those employed or "actively seeking work."

Another departure in Kernell's replication is the use of the eighteen-year-old and over population as the voting age population for computations in connection with the 1971 election, even though the voting age was lowered only a few months prior to the election. Since the younger generation is likely to be generally less responsive to coercion than the older one, the inclusion of this younger element in the computations somewhat prematurely probably understates the influence of the fear-related variables.

Finally, although it is not as central to our own analysis, Kernell's decision to use the Humphrey vote and the Evers vote to measure black turnout in the white majority counties strikes us as somewhat dubious, despite the high correlation between these two votes. In the absence of alternative evidence, the suspicion remains that in counties where the black population is small, local whites might feel at ease enough to vote their economic self-interest and support an economic liberal. Otherwise we would have to believe, for example, that an astounding 76 per cent of the 1,199 voting age blacks in Harrison County trooped to the polls to vote for Hubert Humphrey in 1968. Certainly a substantial share of these votes must have been cast by whites.

and hence assess more accurately the utility of our original models in accounting for the variations in black turnout during the more recent 1971 elections, we reproduced the original analysis using 1970 values for our independent variables and 1971 values for the dependent variable.[4] As in our original analysis, we restricted our attention to the same 29 substantially black counties in Mississippi on the grounds, outlined above, that blacks in these counties experience pressures significantly different from those felt by blacks in the substantially white counties. In addition, since we continue to have faith in the validity of the independent variables in our original analysis for examining the 1967 and 1968 elections, focusing on the same counties permitted us to measure empirically any change in participation patterns that might have occurred over time. To the extent that fear declines as an explanation of black turnout between 1967 and 1971, for example, we can speak of an "opening" of the political system, signified by the displacement of "fear-based nonparticipation" by "apathy-based nonparticipation."

The results of the up-dating of our original analysis, reported in Table 2, are thus doubly interesting. In the first place, they indicate that in 1971, as in 1967–68, the "expanded fear model" continues to explain more of the variance in black turnout rates than the "apathy model."[5] In the second place, however, they suggest that the relative power of the fear model has declined over time: instead of the 3:1 advantage over the "apathy model" in terms of variance explained that it enjoyed in the latter 1960s, the fear model holds a far slimmer 11:9 advantage by the early 1970s. Apparently some important changes have occurred in Mississippi political life since the first half decade following the Voting Rights Act of 1965. Perhaps the most important feature of these findings for analytical purposes, however, is that our indices permit us to measure these changes empirically. In the process, they point the way to further research into an important dimension of political change, not only in the black-belt South but also in other regions of the world where comparable situations exist. Indeed, the transition from what we have termed "fear-based nonparticipation" to "apathy-based nonparticipation" might serve as a fruitful new way to assess the pace of "political development" in so-called developing regions.

The conclusion that emerges from our aggregate analysis of Mississippi black-belt counties to the effect that fear of coercion continues to influence turnout patterns substantially finds additional support, moreover, in individual level data as well. Prodded by Mr. Kernell, and by our own suspicions about aggregate data, we rechecked the relationship between economic coercion and black voting using survey data from the 1968 and 1970 Michigan Survey Research Center opinion surveys. Unfortunately, the Survey Research data is not wholly satisfactory, since SRC polled only 200 Southern blacks whose voting and occupational status could be determined and only 117 of these lived in counties with populations over 30 per cent black. Keeping in mind the inherent limitations of so small a sample and the special limitations of survey research in tapping the political attitudes and behavior of southern blacks, some interesting results nevertheless appear when we divide the SRC sample into vulnerable and invulnerable groups as we did for our Mississippi study. In particular, the turnout rate of the invulnerable group in the 1968 and 1970 elections was significantly higher than that of the vulnerable group, as our "fear model" would predict. (See Table 3.) What is more, the spread between the two groups is considerably greater in counties with substantial (over 30 per cent) black concentrations than in counties without such concentration, also as our fear model would predict. Finally, and perhaps most interestingly, when we applied the same occupational classifications to deep-southern whites in the SRC survey to determine whether we might actually be observing the effects of occupational status or some other effect of occupation not linked to coercion and therefore applicable

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[4] The data for the independent variables came from *U.S. Census of Population, 1970, General Social and Economic Characteristics: Mississippi, No. 26;* and the *U.S. Census of Agriculture, 1969,* Vol. 1, Area Reports, Part 33, Mississippi. In addition, we used the "organizer scores" collected earlier in connection with the original research for our article.

Unfortunately, the Socio-Economic Status (SES) index used to measure occupational status in our original "apathy" model has not yet been computed for 1970 and had to be dropped from the analysis. Given the high correlation between the SES index and education, however, the effect of this is probably slight.

We were also unable to include in our vulnerability index the workers employed outside their county of residence, since figures on this variable were not reported in the 1970 Census.

Except for these alterations, the analysis presented here mirrors that reported in our original article.

[5] When these models are computed using the twenty-one-year-old and over—instead of the eighteen-year-old and over—population as the base, the "spread" between these two models increases, with the fear model accounting for 55 per cent of the variance and the apathy model for 45 per cent. Since the voting age was lowered to 18 only a few months before the 1971 election, there is reason to believe the twenty-one and over population is the more accurate one to use. We have nevertheless reported the results using the eighteen and over population in our Table 2 in order to conform to Mr. Kernell's methodology.

Table 3. Voter Turnout as a Function of Economic Vulnerability Among a Sample of Southern Black Interviewees

	% Voting, 1968 and 1970		
	Entire South N=200	Heavily Black Counties N=117	Heavily White Counties N=72
Invulnerable	68% (N=44)	69% (N=26)	67% (N=18)
Vulnerable	51% (N=156)	48% (N=91)	57% (N=54)

alike to whites and blacks, the results are far different. Deep-southern whites, as predicted, do *not* respond to occupational classifications as southern blacks do. Indeed, to the extent that there is any occupation-related difference in the turnout rates of southern whites apparent in the data, it is the "vulnerable" whites who participated more, reversing the relationship found among blacks. (See Table 4.)

Given the limitations of this SRC sample, we cannot claim these results as conclusive evidence of the importance of economic coercion among southern blacks. We offer them, therefore, only as a partial independent confirmation at the individual level of a set of relationships our article and this comment established at the aggregate level. As such, they are still most encouraging.

One final comment is in order concerning the interpretation of the data reported in our Table 2 above. Like Mr. Kernell, we discover a larger Beta weight coefficient for the median education variable than for any other, even under controls. Not only does this raise again the problem we discussed earlier of how to interpret the education variable, but also it raises centrally the whole question of how to treat Beta weights in evaluating policy alternatives. Mr. Kernell questions the policy implications we draw from our analysis on grounds that the Beta weight associated with the education variable is greater than that associated with the economic vulnerability variable, and that a greater payoff in terms of increased black voter turnout would therefore accrue from concentrating resources on altering education levels rather than on altering economic vulnerability levels. Beta weights, however, indicate only the proportion of a standard deviation change in a dependent variable produced by one standard deviation change in the independent variable. Because different independent variables have different standard deviations, however, before embracing or rejecting any particular policy recommendation it is imperative to examine two facts in addition to the Beta weight: first, the actual sizes of the standard deviations of the various variables; and second, the "cost" of achieving a one standard deviation change in these variables. The first of these is readily available, but the second is far more elusive. No comprehensive and conclusive statement can be made here, therefore, about the relative advantages and disadvantages of the two policy alternatives arising from both Kernell's and our own research. Nevertheless, some preliminary scouting of the terrain is possible. In particular, the data indicate that to achieve a one standard deviation change in black median education in a county would require raising the median education level of the county's entire adult black population by an average of 8/10 of a year, or almost a full grade level (using 1970 figures). By contrast, a one standard deviation change in the proportion of a county's black population most vulnerable to economic coercion would require shifting only 6 per cent of the county's adult black population out of this vulnerable condition. Since, as we argue, this latter result could be at least partially achieved through basic administrative changes in the welfare system with relatively little additional expenditure compared to the costs of additional education, we continue to believe there is a strong argument on "cost effective" grounds alone for pursuing policies aimed at relieving vulnerability, despite the greater Beta weight associated with the education variable. Clearly, however, this conclusion must remain tentative pending further detailed analysis.

Table 4. Voter Turnout as a Function of Economic Vulnerability Among a Sample of Deep-Southern White Interviewees

	% Voting, 1968 and 1970 (N=141)
Invulnerable	59% (N=34)
Vulnerable	64% (N=107)

Federal Registrars and White Turnout

In addition to his direct comments on our article, Mr. Kernell presents two further pieces of analysis that bear on the general subject of black political effectiveness in the South and that require brief comment here. The first is a more detailed examination of the impact on black turnout of the proportion black in a county. The second is an examination of the impact of black voter turnout on white voter turnout. In both cases, Mr. Kernell develops interesting findings that are quite consistent with our own earlier hypotheses. In both cases, however, he also encounters some serious difficulties.

So far as the former is concerned, Kernell confirms and strengthens our earlier finding that high black population majorities are associated with high black turnout rates, largely because of the effect substantial black majorities have as organizational resources. But when he attempts to measure the impact of political organization directly in mobilizing the electorate, Kernell comes perilously close to a tautology. This is so because the indicator of movement activity and organization that he chooses—the presence of federal registrars—is by Kernell's own admission "an important ingredient in black political mobilization" (footnote 17).[6] What Kernell fails to do, however, is specify why this should be so. His use of the registrar figures as a surrogate for local organization implies an assumption that the presence of a federal registrar signifies the existence of a strong, local black political organization. The triggering mechanism of the Voting Rights Act, however, was an administrative finding of *low* black registration, which would suggest that registrars were sent where black organization was weakest. Nor did the registrars create organizations or seek to dissipate apathy by stimulating voter registration drives. Rather, their role was completely passive: to appear at the courthouse and register eligible voters who presented themselves. Finally, Kernell's interpretation of the registrar figures overlooks the numerous counties (like Sunflower County, Mississippi) in which Movement activity was heavy but to which registrars were never dispatched until after 1968.

An alternative, and we think more plausible, explanation of the positive relationship Mr. Kernell finds between the presence of federal registrars and black turnout might be that the registrars' presence dissipated *fear* by demonstrating, at long last, the federal government's apparent intention to protect black voters, and to do so at the local level where the protection was really needed. Federal registrars provided tangible evidence of federal support for the expansion of black political participation and thus probably constituted an important source of confidence to fearful potential black voters. In short, Mr. Kernell's findings here seem to confirm our own conclusions about the role of fear in black participation.

Kernell's demonstration that white voting turnout tends to increase with increases in black turnout is equally intriguing, but also somewhat troubling. The problem here is that it is difficult to determine how much confidence to put in the results since Kernell neglects to carry over into his own analysis of white turnout the same empirical precision he brought to bear in evaluating our analysis of black turnout. In particular, no controls are introduced to determine whether the sole independent variable, black turnout, that is used to explain variations in white turnout is really a surrogate for some other factor altogether that is related to *both* black turnout and white turnout. White education levels, for example, tend to be highest in the black-belt counties, which also have the highest black turnout rates, as Kernell has shown. In the absence of careful analysis employing controls, it is difficult to interpret the results with any confidence.

Conclusion

Mr. Kernell has done a service in subjecting our analysis to scrutiny and extending its reach over time. For the most part, we find his detailed objections to our analysis to be thoughtful but not convincing, for the reasons elaborated at length above. We therefore believe that our mode of analysis survives his scrutiny intact. In fact, in many respects, we believe Mr. Kernell's findings complement ours, particularly in verifying—as we would have predicted—that our findings about the role of fear do *not* seem to apply to white-majority counties; in confirming the importance of the organizational resource represented by high black proportions in a county; and in demonstrating a positive relationship between black turnout and the presence of voter registrars, which we believe can be explained in terms of the registrar's role in dissipating fear. Where we differ from Mr. Kernell is in our evaluation of the accuracy of 1960–64 data for analyzing the 1967 and 1968 elections and in our conclusions about the role of "fear" in influencing black participation both in 1967/68 and in 1971. So far as the latter is concerned, the application of our earlier mode of analysis to the more recent 1971 elections, and a brief examination of available survey data, confirm, we believe, our earlier conclusion about the importance of fear in explaining black turnout in the black-belt counties of Mississippi, even though that importance has diminished somewhat over time. Finally, and perhaps most interestingly, this updating of the analysis suggests the potential utility of the indices and theory we developed in our original article to students of political development interested in studying empirically the whole process of political change associated with the "opening" of political systems to mass popular participation in situations involving extensive social and economic conflict.

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[6] Our own index of civil rights movement activity was explicitly designed to avoid this problem by measuring movement activity directly instead of in terms of its hypothesized results. Yet Mr. Kernell neglects any mention of our organizer index.

COMMUNICATIONS

TO THE EDITOR:

Recently John Ruggie extended some of the tools of economic analysis into the field of international organization "to demonstrate and illustrate one possible theoretical system," specifying "when, where and how states seek to organize internationally,"[1] I believe that the full theoretical implications of his analysis were not drawn upon and that the article could have been substantially enhanced by a simple extension of his model. Let's imagine his same world consisting of two identical states, *A* and *B*, with identical preferences, resources, and capabilities. Resources are of course, limited and may be used in the pursuit of given tasks either domestically or through international cooperation. We will, however, change two of his assumptions in the direction of reality. First, we change his assumption that "decisions made in one context remain independent of decisions made in another," (p. 878) to admit their dependence. It seems highly unrealistic to imagine that country *A* does not consider what is being done for it internationally in deciding what to do for itself on the domestic level. Secondly, that there is "no direct interaction between *A* and *B*" (p. 879) seems very naive in a theory relating to international organization.

Let us now go to Figure 1, which is a slightly modified version of Ruggie's Figure 2. On the vertical axis we measure for only one country, *A*, "Task Performance through International Organizations" (hereinafter referred to as "*i*") and on the horizontal axis "Task Performance through National Organizations" (referred to as "*n*"). A preference map, composed of a set of indifference curves completely describes *A*'s preferences regarding *i* and *n*. The slope of an indifference curve at any point shows the rate at which *A* is willing to give up *i* to obtain *n*. This is referred to as the marginal rate of substitution of *i* for *n* (MRS_{in}). In the diagram we have drawn only two of the infinite number of indifference curves in *A*'s preference map, I_1 and I_2.

A's ability to perform tasks through *i* or *n* depends upon the resources available to *A*. The feasible and efficient combinations of *i* and *n* possible for a given amount of resources are represented by a transformation curve, the slope of which at any point indicates how much more *n* can be obtained by transferring resources from *i* and vice versa. This is referred to as the Marginal Rate of Transformation of *i* to *n* (MRT_{in}). Assume for the moment that *A*'s resources permit the transformation curve T_1 N_1. Given T_1 N_1, *A* will be in equilibrium at E_1 where $MRS_{in} = MRT_{in}$, either of which is represented by the slope of line P_1. The slope of P_1 can be thought of as the implicit "price" of *n* in terms of *i* since it represents the rate at which one is "traded off" for the other. We have assumed that the picture for *B* is exactly the same as described for *A*.

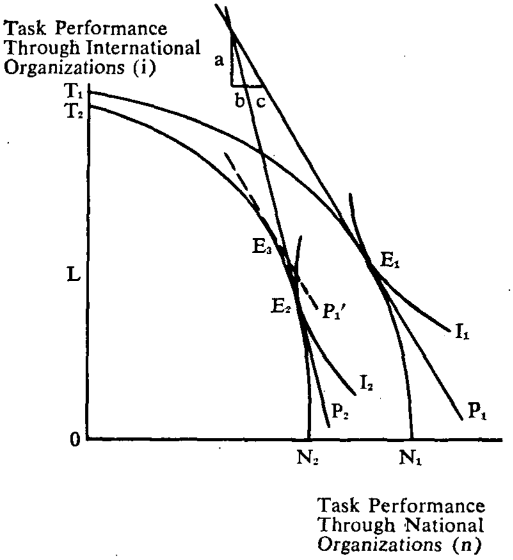

Figure 1. Task Performance through National and International Organizations.

I am in agreement with Ruggie that if we now shrink the resources of *A* (leaving *B* the same), the reduction in *A*'s ability to produce *n* will contract at a greater rate than its ability to produce *i*. This is demonstrated by a drop in the transformation curve to let us say T_2 N_2. The reason behind this is that *B* continues to contribute to international organizations and that many of the things supplied by the international organization are, at least in part, public goods. *A* receives the benefit of *B*'s contribution whether or not *A* contributes. This may be best illustrated in the extreme case where if *A*'s resources fall to zero, its ability to produce *n* is reduced to zero. But some *i* will still be forthcoming from the international public goods supplied exclusively by *B*. The transformation curve for *A* would become a single point on the verticle axis, such as *L*.

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[1] John Gerard Ruggie, "Collective Goods and Future International Collaboration" *American Political Science Review*, 66 (September, 1972), 874–93.

Should there be no interaction between A and B, A will be at a new equilibrium at some point, like E_2, with a new implicit price for i and n represented by the slope of P_2. This is the point at which we are left by Ruggie. Let us take this a step further and open up an interaction between A and B and observe the implicit prices of i and n in both places for they now differ. I have extended P_1 and P_2 to their intersection for comparative purposes. In A, i trades off for n at the rate a/b. In B, the same trade off is $a/b+c$. In terms of "opportunity cost," n is relatively costly in A and i is relatively costly in B.

If i and n were normal commodities, any text in international economics would show that A would begin to specialize in i, B in n, and that through trade both would be better off. For example, if A could "trade" with B at the implicit price ratio existing in B, A would move to "production" at point E_3, where A's MRT_{in} $=$ slope P_1' $=$ slope P_1, thereby taking on greater international responsibilities and acquiring n through international trade. (Indeed the implicit price will not remain equal to the slope of P_1' since as B moves to a point to the right of one like E_1 the "price" of i in terms of n decreases. This modification would add clutter without insight.) Trade would take place along line P_1', raising A to some indifference curve above I_2, until the highest possible indifference curve is reached at which point $MRS_{in} =$ slope $P_1' = MRT_{in}$, thus establishing a new equilibrium. But i and n are not normal commodities and n cannot be provided to A by B since n is explicitly a domestic task. How can n be acquired through trade? Simply by transferring some of the tasks which have been performed domestically into the international sphere. A therefore has the incentive to make some of the tasks which have been performed domestically into tasks which are now performed by international organizations. (If this change is made then the dimensions of the diagram change to reflect that with the same level of resources more i and less n is forthcoming simply because there are now more i tasks and fewer n tasks.)

Performance of domestic tasks as compared to internationally performed tasks becomes *relatively* more difficult for the smaller country, since they are performed entirely out of domestic resources. International tasks on the other hand do receive some support from outside the smaller country. Those tasks aided by the international community become easier to perform *relative* to domestic ones. Therefore if the smaller country can shift some domestic tasks into the international sphere, a greater number of tasks will be performed in total. The smaller country may maintain a preference for domestic task performance but nevertheless as it becomes relatively smaller (in terms of capacity) it feels an even greater incentive to shift tasks onto the international community.

While A has the incentive to shift tasks into the international sphere, doing so is not possible without the cooperation of B. First of all, A is able to present B with its greater willingness to assume international tasks and have some of its tasks assumed by international cooperation as is demonstrated by the willingness of A to move toward point E_3. Secondly, B has had an experience exactly opposite to that of A. B finds that if i and n were normal commodities, it would be to B's advantage to try to specialize in n and obtain i through trade. In our example however, this is equivalent to a willingness on B's part to assume some tasks which were i tasks and make them n tasks. At a time when A is placing greater demands upon the international system the implicit price of internationally performed tasks rises. B's response is to shift toward greater performance of n tasks. Whether or not B will accept international tasks as domestic tasks would depend upon the system operating in B. Neither i nor n are homogeneous items (as is always the case in similar economic analysis). In increasing n, B may perform tasks which may be public goods within B but not between A and B (e.g. public works in B), public goods in A but not in B (aid to A) or public goods in both places (e.g., pollution abatement). The latter two would represent the adoption of international tasks as domestic. Which of these B would choose is the meat of international cooperation and cannot be answered by this analysis.

Whether or not there is a net increase in the participation of international organizations in task performance will depend upon how many new tasks are shifted to the international organizations by the small countries (countries like A) and how many formerly international tasks are adopted as domestic by the larger countries (here, B countries). This is a problem in dynamics and depends largely upon the gap between the "small" countries (size in terms of capabilities) and the "large." If the gap is widening over time we should be observing the "large" countries assuming as domestic some tasks which were international, and the "small" countries simultaneously transferring domestic tasks to the international sphere.

Ruggie's data support the conclusion that the large countries will adopt some international tasks as domestic. His examination of

1960s' data on development assistance from the Development Assistance Committee (DAC) countries of OECD shows a direct relationship between GNP (representing national resources) and the amount of resources being devoted to development assistance. This is an indication that one task (development) which formerly was handled almost exclusively by domestic organizations in the developing country is being shifted at least in part out of the developing country and into the international arena. Second, his data show an inverse relationship between GNP and the ratio of multilateral to bilateral aid contribution. Since these data pertain only to DAC countries—all of which may be classified as "large" (or *B* countries in our above analysis), another hypothesis is given credibility. That is, as a country becomes increasingly "large" it has a tendency to adopt formerly international tasks as part of its own domestic tasks. In this case as the *A* countries shift their development task into the international sphere, the *B* countries are willing to adopt development as one of their domestic objectives and extend increasing amounts of bilateral aid.

My model has indicated an increasing incentive for small countries to shift domestic tasks into the international sphere as the capability gap widens between them and large countries. We would expect negative correlations between the difference: (Developed Country GNP) minus (Developing Country GNP) i.e., the development gap, and the ratio:

$$\frac{\text{Developing Country Contribution to Multilateral Development Efforts}}{\text{Developing Country Contribution to Domestic Development Budget}}$$

To substantiate this theory such results would only be valid in a world where there is this "capability" gap which is widening between the *A* and *B* countries, that is where the production possibilities curves such as those in Figure 1 are growing farther apart. This is of course the case.[2] Generating the data for a test on this relationship would be a considerable task since domestic development budgets would be difficult to disaggregate from overall budgets. Nevertheless, we have rough approximations to verification of this in the fact that only recently have developing countries begun to look to international cooperation as a means to economic development. Only within the past 25 years or so have they begun to contribute in increasing amounts to such organizations as regional development banks, economic integration movements and so on. The numerator in the above ratio has only recently become positive, at a time when the development gap is widening.

What I have attempted to do here is to draw out some of the possible extensions of the Ruggie model so as to create a more dynamic exposition of the framework within which nations interact to decide upon international as opposed to domestic task performance. Indeed the facts that the task categories are not homogeneous and that domestic tasks can be shifted to the international arena complicate the issue considerably.

WILLIAM LOEHR

University of Denver

TO THE EDITOR:

I am pleased that Professor Loehr thought my article to be sufficiently important to warrant a comment on his part; in turn, his comment is sufficiently thoughtful to warrant a response on mine.

Loehr's "extension" of the model presented in "Collective Goods and Future International Collaboration" (*American Political Science Review,* 66 [September, 1972], 874–893) stems from his desire to change what he takes to be two of my assumptions "in the direction of reality." I must point out, however, that, perhaps because of lack of clarity in the original formulation, he misconstrues both assumptions he wishes to change. First, he interprets the assumption that "decisions made in one context remain independent of decisions made in another" to mean that what country *A* does domestically in any given sector will remain independent of what it will seek to do internationally in that sector. It was not my intention to convey such an impression. In fact, the entire first half of the article, including Figures 1 and 2, Propositions 2 and 3, as well as Tables 1 and 2, was designed to show precisely how the domestic and international arenas are related to one another as a country decides upon an appropriate level of international collaboration. What the assumption means, as is stated at several points in the discussion, is that decisions in different functional contexts, such as public health, the regulation of nuclear proliferation, and monetary affairs, will be made on the basis of issue-specific criteria, so that *these* contexts remain independent of one another.

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[2] Everett E. Hagen and Oli Hawrylyshyn "Analysis of World Income and Growth, 1955–1965," *Economic Development and Cultural Change,* 18 (October, 1969), 42–63.

Second, Loehr wishes to drop the assumption that there is "no direct interaction between *A* and *B*" (where *A* and *B* are two countries), thinking it to be naive when theorizing about international organization. I did too. The assumption is explicitly dropped on p. 886, and the entire second half of the article is devoted to conceptualizing and illustrating the consequences of different types of interdependencies in the relationships between *A* and *B*. For some inexplicable reason, Professor Loehr ignores this section altogether.

Loehr's comment, in other words, is not so much an extension of my model based on different assumptions, as it is an alternative formulation based on the identical assumptions. As such, it is an interesting contribution with which I would have but minor quarrels, which need not be pursued in this forum.

John Gerard Ruggie
University of California, Berkeley

To the Editor:

The review of my book, *The Politics of Reform in Peru,* in the March 1972 issue of the *American Political Science Review,* overlooked an essential point in criticizing certain areas of omission. Contrary to the reviewer's assumption that this was primarily a book about APRA and Peru, I was trying to discover the role of the mass reform party as an instrument for initiating reform. Regardless of emotional attitudes toward the Apristas of the present, they did provide some of the best historical evidence available concerning this connection. The reviewer failed to discern how "development" was relevant, because he regarded the party as the point of the book. My intention was to use APRA (and, in less depth, AD in Venezuela and MNR in Bolivia) as the source of independent variables, in order to trace the apparent effects on developmental reforms as dependent variables. How well I succeeded is, of course, open to debate, but let the book be judged on the basis of its design, and let other books be written by those who prefer different approaches.

Grant Hilliker
Ohio State University

To the Editor:

While I much appreciate Professor Rudolf Heberle's extensive review of *From Karl Mannheim* in the September, 1972 *American Political Science Review,* I should like to point out that his statement that "The essays have been available in English before, except one which is given in Wolff's translation (Problems of Sociology in Germany)" is not quite correct. Several other items have never before appeared in translation. There is another essay "On the Diagnosis of Our Time," which was printed in the first issue of the shortlived *Mass und Wert,* coedited by Thomas Mann (1937). And there are, in the Introduction, the translation of the original analytical table of contents, three closely printed pages of *Ideologie und Utopie,* neither translated nor reprinted in German in later editions (Sec. XVI); an excerpt from an untranslated small book on the "tasks of sociology called for by the present—a teaching program" of 1932 (Sec. XVI, 1); and the translation of Mannheim's answer to a newspaper poll of 1937 (Sec. XIX, 2). It should also be pointed out that the book is available not only in the hardcover edition for $12.50 but also as a Galaxy paperback for $3.95.

Kurt H. Wolff
Brandeis University

To the Editor:

I wonder if I might venture a comment on Edward N. Muller's impressive "A Test of a Partial Theory of Potential for Political Violence" (*American Political Science Review,* 66 [September, 1972], 928–959). Though ably conceived and rigorously executed, the article seems to claim a bit too much.

The central thrust of the article is to debunk Ted Robert Gurr's theory of relative deprivation (RD) in favor of an "alternative approach" resting on two variables: (1) the perceived legitimacy of the regime, (2) the perceived viability of political violence in attaining objectives. However, as Professor Muller notes, legitimacy is one of four intervening variables in Gurr's earlier formulation of RD theory (the other three being facilitation, institutionalization, and the coercive potential of the regime), with RD itself as the independent variable and violence as the dependent variable (Gurr, "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review,* December 1968). Although there are several references to Gurr's *Why Men Rebel* (1970), Professor Muller does not refer to Gurr's further refinement in that work of his conceptualization of the intervening variables. In Gurr's revised formulation, whether RD explodes into actual violence becomes the function of two sets of intervening variables: (1) the scope and intensity of normative (historical, cultural, ideological) justifications for violence as well as *men's perception of the effectiveness and utility of violence in solving social problems;* (2) the relative physical strength of the contending parties as well as their ability to provide institutionalized, peaceful alternatives to violent expression of discontent.

Moreover, Professor Muller seems to overlook the point that RD, though fundamentally a psychological concept, may have a variety of bases. The most common is the economic, in which RD refers to men's perception of discrepancy between expectation and achievement. Equally important is the political base, in which RD refers to men's perception of discrepancy between their socioeconomic status and political influence. In fact, John T. McAlister, Jr., has applied to the Vietminh Revolution Gurr's model of relative deprivation as a basic condition of political violence. He identifies a group of relatively well-educated Vietnamese whose socioeconomic advance was thwarted by inadequate employment opportunities. He focuses on a second group of native intelligentsia who had achieved a degree of socioeconomic advance but no corresponding political power. McAlister concludes that the denial of political power to the intelligentsia—and the resultant discrepancy between socioeconomic status and political influence—was a most important source of discontent leading to revolution. He believes that much of the impetus for the Vietminh Revolution came from the relatively small segment of the Vietnamese population who had experienced some social mobility in the colonial contest, but no commensurate political power (McAlister, *Vietnam: The Origins of Revolution,* 1970, esp. pp. 325ff.).

In short, it is difficult to see how Professor Muller's treatment is an "alternative approach" to Gurr's. Would it not be more accurate to say that Muller has selected for extended treatment two of Gurr's intervening variables? (Could it not be, for example, that the erosion of the "legitimacy sentiment," as Professor Muller puts it, is due to relative deprivation?) In that case, are not the Gurr and Muller approaches complementary rather than mutually exclusive?

MOSTAFA REJAI

Miami University

TO THE EDITOR:

Professor Rejai's claim that my partial theory is not an alternative to Gurr's theory is correct—if one interprets the words "alternate approach" (p. 930) in my article to mean *mutually exclusive*. Probably, I should have specified what I meant by "alternate approach" more precisely; but, I assumed it was clear enough from the context of the article as a whole. I did not intend to equate "alternative" with "mutually exclusive;" rather, I meant to call attention to the difference in emphasis between Gurr's theory, which proposes that relative deprivation is the root cause of potential for political violence (*Why Men Rebel,* 1970, pp. 24, 36–37, 159, 328), and my partial theory, which proposes that relative deprivation is not one of the major causes of potential for political violence, and is considerably less important than the diffuse support/legitimacy/political trust concept. My partial theory is definitely complementary to Gurr's in the following ways: (1) Legitimacy is an important concept in both formulations; (2) my partial theory includes the hypothesis, suggested by Gurr (p. 193) that shared discontents (presumably relative deprivation) are only related to potential for political violence via legitimacy, i.e., have no direct effect on potential for political violence independent of legitimacy (which, by the way, seems to be inconsistent with the relative deprivation → potential for collective violence → potential for political violence sequence conjectured at pages 24, and 159 and depicted in the causal model at page 328); and (3) belief in the utility of past violence is an important concept in both formulations.

Actually, in a presentation of a full theory of potential for political violence ("A Nonalienation Interaction Theory of Political Protest," paper prepared for the European Consortium for Political Research Joint Sessions of Workshops, "Political Behaviour, Dissatisfaction and Protest," Mannheim, Germany, April 12–18, 1973), I hypothesize that specifically *politicized* relative deprivation—relative deprivation that is blamed on political and social structures —*is* an important direct cause of potential for political violence, when it interacts with other variables. On the other hand, I still do not conceive of relative deprivation, even in a "politicized" form, as the root cause of potential for political violence, with Legitimacy & Co, as intervening variables.

EDWARD N. MULLER

SUNY at Stony Brook

On Understanding Fascism: A Review of Some Contemporary Literature*

A. JAMES GREGOR

University of California, Berkeley

The above collection of recently published books provides a convenient occasion for undertaking a review of our efforts to "understand" generic fascism—and particularly Mussolini's Fascism. For almost fifty years now, biographers, historians, political scientists, and social scientists in general, have been attempting to "understand," in some substantial sense, the "meaning" of the tangled skein of events we now identify with fascism. Whatever "understanding" is taken to mean, it implies, minimally, that we can *define* our objects of inquiry with some precision—that we entertain some confirmed empirical *generalizations* about them—and that these generalizations be *logically related* to produce a comprehensible narrative account. Such an account is generally spoken of as an "empirical theory" of our range of concerns. Preliminary to, implicit in, and consequent upon, the possession of such a theory, are *normative* or *evaluative considerations* or both. In general, it can be said that to "understand" a complex political sequence, one is expected to have, at his disposal, sets of such formulations available for public display and intersubjective scrutiny.[1]

Social scientists have employed a variety of intellectual strategies for coming to understand complex historical and political phenomena in this way. These strategies have included interpretive biographies, special accounts of one or another aspect of a complex sequence of events, broad-gauged narratives devoted to the general history of the phenomena in question, as well as general theoretical approaches and special and specific theories calculated to explain them. Our collection includes instances of all these strategies: a major biographical work (by Richard Collier); special accounts of American public opinion concerning Fascism (by John Diggins) and the relationship between Italian business leadership and Fascism (by Roland Sarti); a standard historical treatment of fascist movements (by F. L. Carsten); two books by historians that attempt to put the entire sequence in theoretical perspective (the works of Gilbert Allardyce and H. Roderick Kedward); as well as three books devoted to "classic" and special theories of fascism (the neopsychoanalytic interpretation of Wilhelm Reich, the moral crisis thesis of Peter Drucker, and the neo-Marxist assessment of Reinhard Kuehnl). Each of them aspires to provide something like adequate understanding of generic fascism in general, and Mussolini's Fascism in particular.

Biography and the Understanding of Fascism

Richard Collier's *Duce!* is an effort to provide understanding of Italian Fascism by revealing something of the personal character of its leader. Collier tells us interesting and insightful things about Mussolini which may be entertaining historical gossip, but which provide precious little leverage on understanding Mussolini or Fascism in any significant theoretical sense. What seems to hold Collier's entire connected narrative together is his penchant for

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The Place of Fascism in European History. Edited and with an Introduction by Gilbert Allardyce. (Englewood Cliffs, N.J.: Prentice-Hall, 1971. Pp. 178. $2.45.)

The Rise of Fascism. By F. L. Carsten. (Berkeley: University of California Press, 1967. Pp. 256. $6.50.)

Duce! A Biography of Benito Mussolini. By Richard Collier. (New York: Viking, 1971. Pp. 447. $12.50.)

Mussolini and Fascism: The View from America. By John P. Diggins. (Princeton: Princeton University, 1972. Pp. 524. $16.50.)

The End of Economic Man: The Origins of Totalitarianism. By Peter F. Drucker. (First Colophon Edition. New York: Harper & Row, 1969. Pp. 271. $1.95.)

Fascism in Western Europe, 1900–45. By H. Roderick Kedward. (New York: New York University Press, 1971. Pp. 260. No price given.)

Formen Buergerlicher Herrschaft: Liberalismus-Faschismus. By Reinhard Kuehnl. (Hamburg: Rowolt, 1971. Pp. 190. DM 3.80.)

The Mass Psychology of Fascism. By Wilhelm Reich. (Newly translated by Vincent R. Carfagno. New York: Farrar, Straus & Giroux, 1970. Pp. 400. $3.25.)

Fascism and the Industrial Leadership in Italy, 1919–1940. By Roland Sarti. (Berkeley: University of California Press, 1971. Pp. 154. $8.00.)

[1] For a more protracted account of what "understanding" might be taken to mean cf. A. J. Gregor, *An Introduction to Metapolitics* (New York: Free Press, 1971), chap. 7.

explaining the behavior of his protagonists in terms of general dispositions to behave as they do. Mussolini behaved as he did largely because he was afflicted with an "unwavering" aim: "to command attention at all costs" (p. 37). Furthermore, Mussolini had developed, according to Collier, an "obsessional life-long hatred of the rich" (p. 41). Between his longing for "personal recognition and prestige" (p. 42), his "gnawing ambition" (p. 45), his "built-in instinct for violence" (p. 45) and his "temperamental" activist disposition (p. 48), Mussolini acted out an elaborate psychodrama using, as a background, the props that history identifies as Fascism.

Whatever understanding such accounts deliver, it is parasitic on ill-contrived psychological generalizations and some curious notions about social dynamics. We do not know, for instance, if we are to take the talk of "instincts" and "obsessions" seriously. But even if we consider such talk nothing more than thoughtless turns of speech, explanations in terms of noninstinctual and nonobsessional psychological dispositions are extremely difficult to warrant. Finally, even if we could produce an empirical warrant for such explanations of behavior, we would still face the problem of explaining why individual psychological dispositions can be acted out on a national and world scale.

Collier, himself, makes it particularly difficult to establish the truth of his own interpretation. We are never quite sure what evidence Collier is prepared to accept in identifying the dispositional traits used in his accounts. As a case in point, he goes to some lengths to record significant instances of Mussolini's personal courage, beginning with his youthful defiance of a farmer's threatening shotgun, through his quietly suffering twenty-seven surgical operations (all but two without anesthetics) to remove bits of shell casing from his body, to Mussolini's calm reaction to various assassination attempts and his near brush with death during his rescue in 1943 (pp. 36, 50–51, 57, 90–91, 268). With such evidence at our disposal we might have imagined that "courage" might be a dispositional property that could be attributed to the *Duce*. And on page 36 Collier seems to grant as much. Mussolini, he tells us, "was no coward." And yet we are mystified to find (on page 361) that when Mussolini faced his assassin, calmly opening his jacket to say "Shoot me in the chest," Collier can only suggest that that display represented "the first time in his life [in which Mussolini] had risen above fear"! Mussolini was, according to Collier, dispositionally "fearful"—and only on the occasion of his assassination did he succeed in rising above it. Curious.

The issue of Mussolini's cowardice is not the only occasion for such a curious treatment of evidence. Collier goes on to tell us, for instance, that the young Mussolini would "often" spend "hours astride an old farm horse in the midst of the swift-flowing River Rabbi, so riveted to a book that only twilight reminded him to head for home" (p. 35), and further, that as a young man, Mussolini would remain "immersed for hours in . . . books" (p. 39). One might imagine that as a consequence, and on the basis of such evidence, the young Mussolini was something of an intellectual. Not at all. Collier informs us that for all his immersion in, and riveting to, books, Mussolini was "always a superficial reader." Mussolini, in fact seems to have read only nine pages of any book—"the first three, the last three, and the three in the middle." Not only that, but his reading was, again according to Collier, very selective. Mussolini, we are told, "read only those writers who spoke for violence, like Georges Sorel and Friedrich Nietzsche" (p. 46).

If we are expected to give credence to such accounts it would seem that one might provide more substantial evidence in their support. Since at least 1964 Mussolini's youthful literary work, published between 1901 and 1914, has been available for scholarly scrutiny. During the historical period under consideration, Mussolini reviewed, for a variety of publications, a large number of books ranging from Russian novels to Robert Michels's socioeconomic treatise on *Cooperation*. Beside these reviews, Mussolini wrote three long monographs and a novel. He translated at least eighteen pieces from the French and the German, including pamphlets by Karl Kautsky, Wilhelm Liebknecht, and Peter Kropotkin. In his various articles Mussolini cited every work then available by Karl Marx and Friedrich Engels—and provided quotations from Marx's *Contribution to the Critique of Political Economy*, the volumes of Marx-Engels correspondence published in 1913, Marx's articles published in the *New York Tribune*, Engels's volume on the *Conditions of the Working Class in England in 1845* and the *Anti-Duehring* as well as Engels's famous introduction to Marx's *Class Struggles in France*.[2] In his monograph on the Trentino (*Il*

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[2] Cf. A. J. Gregor, *The Ideology of Fascism* (New York: Free Press, 1969), pp. 95f. Any reasonably adequate assessment of Mussolini's intellectual capabilities would require an acquaintance with his collected works now available as the *Opera omnia*, 36 volumes (Florence: La fenice, 1951–1963). In the

Trentino veduta da un socialista), Mussolini provided extensive quotations from H. S. Chamberlain's *Grundlagen des neunzehnten Jahrhunderts*. In his monograph on John Huss (*Giovanni Huss il Veridico*) Mussolini quoted extensively from the work of Friedrich von Bezhold on the history of the Reformation in Germany—and from the works of L. Leger, C. Cantú—as well as those of Huss himself. In his polemics against Alfredo Taglialatela, *L'uomo e la Divinità*, published when Mussolini was twenty-one, one can find verbatim quotations from a variety of ethical and philosophic works.

Whether all this is simply the product of "superficial reading" requires more in terms of evidence and analysis than Collier's insistent pronouncement. That it was the product of just having read nine pages of each book is more than doubtful. Appropriate quotations are not usually culled from the first three, the last three and the middle three pages of any book. That Mussolini read *only* authors who "spoke for violence" seems to be simple nonsense.

If we are to come to "understand" Fascism by coming to understand its *Duce*—and understanding him implies that we entertain defensible generalizations about his dispositional traits—it would seem that Collier gives us little intellectual leverage. Collier's book is, in fact, representative of an entire genre of biographical literature devoted to the interpretation of Fascism by coming to "understand" its leaders.[3] What is generally missing from such idiographic accounts is theoretical substance. There are no defensible generalizations that bind the instantial descriptions together. Collier, as the case in point, offers us little more than a collection of interesting vignettes shaped out of the memories of the hundreds of participants in the Fascist sequence whom Collier has interviewed twenty-five years after the events they are asked to recall. With a research staff that at its peak totaled forty-four, involving an enterprise that covered seven countries, consumed three-and-a-half years of time, and elicited the recollections of 454 eyewitnesses, one might have legitimately expected a great deal more.

Biographies seem to be afflicted with special kinds of disabilities. This may be because the authors of biographies believe that since they are dealing with an individual life they need not search out defensible generalizations that might successfully lace their account together. Irrespective of that seeming conviction, it is obvious that biographies, in their pursuit of understanding, generally exploit unrestricted but poorly formulated, generalizations about their subjects' psychic or emotional dispositional traits. In this respect Collier's volume is simply one instance in a large class of similar volumes. Its shortcomings are those of the entire class.

All this is not to say that such disabilities uniquely afflict biographies. John Diggins's recent volume concerning American attitudes toward Fascism seems to suffer from similar deficiencies.

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bibliography appended to his book, Collier refers (p. 410) to Mussolini's *Opera omnia* as composed of "23 volumes," which suggests that he is not well acquainted with the work. Furthermore, on the same page Collier cites the pre-war collection of Mussolini's speeches and essays, the *Scritti e discorsi*, as involving only 12 volumes when they, in fact, involve 13—and he gives "La fenice" as the publisher when the publisher was, in fact, Hoepli.

[3] It would seem that Alan Cassels (*Fascist Italy* [New York: Crowell, 1968] pp. 1f.) is correct in suggesting that because we have little serious understanding of the entire fascist phenomenon, we attempt to search out the "meaning" of it all in biographies devoted to fascist leaders. There is an incredible number of biographies devoted to Mussolini—and most of them, largely because they serve as surrogates to serious understanding, are hopelessly incompetent or transparently biased or both. The best biography of Mussolini is, of course, the as yet incomplete work of Renzo De Felice (*Mussolini il rivoluzionario* [Turin: Einaudi, 1965], *Mussolini il fascista: la conquista del potere, 1921–1925* [Turin: Einaudi, 1966], *Mussolini il fascista: l'organizazione dello stato fascista, 1925–1929* [Turin: Einaudi, 1968]). The philofascist biographies of Giorgio Pini and Duilio Susmel (*Mussolini: l'uomo e l'opera*, 4 volumes [Florence: La fenice, 1953–55]) and Ivon de Begnac (*Vita di Benito Mussolini*, 3 volumes [Milan: Mondadori, 1936–40]) are valuable for scholarly purposes: they undertake a careful review of the available historic evidence in their efforts to expose pervasive bias and scholarly irresponsibility in the academic treatment of Fascism. In this respect Bruno Spampanato's *Contromemoriale*, 3 volumes (Rome: Illustrato, 1952) is not as useful, beset as it is by selective reporting of evidence and some significant historical inaccuracies. Most of the biographies available in English are all but totally inadequate. Most are written by nonacademics and are animated by an irrepressible bias. Those of Paolo Monelli, *Mussolini: The Intimate Life of a Demagogue* (New York: Vanguard, 1954) and Laura Fermi, *Mussolini* (Chicago: University of Chicago, 1961), are among the worst. It is interesting, in this regard, to compare the "official" biography written by Giorgio Pini while he served the Fascist Regime, *The Official Life of Benito Mussolini* (London: Hutchinson, 1939), with the postwar biography he wrote with Diulio Susmel. Of the biographies available in English, Christofer Hibbert's, *Benito Mussolini: The Rise and Fall of Il Duce* (Baltimore: Penguin, 1962) is among the best. We do not have, as yet, a fully objective and competent biography of Mussolini in English.

Special Interpretive Histories and the Understanding of Fascism

For our purposes, John P. Diggins's *Mussolini and Fascism,* is an instructive book. While its principal object is to reveal *something* about the prewar American views concerning Mussolini and Fascism (which it does quite well), it attempts a great deal more. Buried in Diggins's account is an implicit judgment that Americans had somehow "misunderstood the essence of the Fascist movement" (p. xv). Diggins admits that Fascism was "an amorphous phenomenon" and that "few American thinkers could define adequately the nature of Fascism" (pp. xvi, 445)—and he grants that until the end of the Second World War there was "no consensus on the nature of Fascism" (p. 365). But for all that, he alludes to "the reality of Fascism" and "the real nature of Fascism" (pp. 483, 504) as though behind the amorphousness and the fugitive definitions there was an "essence" that Americans *should* have perceived. Presumably we have, today, espied that "essence." Unhappily, a characterization of that "essence" is scarcely to be found in the five hundred pages of Diggins's closely written prose.

It is clear, of course, that for Diggins, Fascism was a "Bad Thing." This judgment seems to satisfy his moral sensibilities. But if we attempt to discover how Diggins would characterize Fascism, we find little more than an identification of Fascism with "totalitarianism" (pp. 25, 190, 197, 251, 334, 435, 456, 464, 466, 467, 487). Beyond that, Fascism was a "right-wing," conservative and reactionary movement that had some "progressive features" and seemed to enjoy a "vast popularity" (pp. 16, 24, 59, 87, 219). It was "right-wing," in Diggins's judgment, at least in part because it exploited "national humiliation—the vital mental germ of almost every right-wing movement" (p. 87). It seems to have been "conservative" and/or "reactionary" because it was "the first anti-democratic political experiment in the twentieth century" (p. 207). Furthermore, Fascism was "the first modern political movement to blare the noise of ideology into concert halls, art galleries, and literary salons" (pp. 240f.).

One can only suggest that none of this is very helpful in trying to characterize the "essence" of Fascism. Not only does "totalitarianism" remain undefined in Diggin's book, but if "national humiliation" serves to spark "right-wing movements," then surely Castro's and Mao's communism seem to qualify as "right-wing." Further, if Diggins suggests that Fascism was the "first anti-democratic political experiment of the twentieth century," he seems to do an historic disservice to Bolshevism. Whatever else it was, Bolshevism doesn't seem to qualify as a "democratic" political experiment. Similarly, to insist that Fascism was the first movement to make art and culture political is to neglect the Bolshevik insistence that all culture serve the Revolution. In any event, it is reasonably clear that Diggins has little to suggest in terms of characterizing the "nature," the "reality" or the "essence" of Fascism. At the end we are left with the commonplace that Fascism is "a state of mind," and that we must "analyze the fascism within ourselves" (p. 495).

Other than these general disabilities, the critical shortcoming that characterizes Diggins's effort turns on his apparent readiness to generalize on the basis of a vague and suspect methodology. Diggins insists upon generalizing about Fascism's "amazing popularity in America" (p. 41; cf. p. xv)—about the "favorable press" Fascism received in America. And yet, on the other hand, he admits that the "American mind" harbored "many-sided and contradictory images" of Fascism and Mussolini. More than that, Diggins is prepared to admit that Fascists felt that a "dreadful misunderstanding" about Fascism was "prevalent" in the United States (cf. pp. 49, 52).

The fact is that there is no public way of choosing between these apparently incompatible generalizations. Diggins admits that he has attempted to generalize on the basis of uncontrolled data, using a methodology that is, at best, obscure. He informs his readers, for example, that he has little confidence in the results of social science survey data and systematic content analysis. Polls, he tells us, provide only "crude opinion surveys" that lack "depth" (cf. pp. xvii, 339). They tend to be "incomplete and unimaginatively formulated . . ." (p. 340). Moreover, "the hard fact . . . is the fetish of the statistician, not the historian" (p. 155). The historian, in his effort to coin generalizations, apparently invests his confidence in something Diggins calls "rough content analysis" (p. xvi).

If all that is true (and it probably is), one can only wonder how, in fact, the historian proceeds to formulate and support generalizations. Diggins recognizes that he has "methodological" problems. He is dealing with that "slippery beast known as public opinion" and the problem of attributing modal values to complex populations and subpopulations. Diggins wishes to transport his readers "to the heart of American political values and assumptions." He intends to talk significantly about "the American mind," the "public mind," the "popular mind," as well as the "nation's political psyche." He will inform us concerning "the values of America's business culture," the "businessman's creed" and the "liberal mind." He will discuss the collective properties of the "American press," characterize a whole nation's opinions as "America's apologia" for Fascism, and allude to the whole nation's response to Fascism as "America's receptivity" (pp. xvii, 6, 11, 19, 21, 36, 43, 60, 68, 73, 158, 167, 221, 291, 338, 361). Most social scientists would be fascinated to know how all this might be accomplished. If systematic content analysis will not do—can "rough content analysis" succeed? If probability sampling and polling won't do, how does one proceed? If the controlled sampling of "hard data" cannot support inverse projection to total populations, what will?

Diggins has surveyed an uncontrolled sample of an indeterminate body of surviving literature, using vague and unspecified criteria of selection and reporting, and has delivered his personal judgments not only about complex constructs like the "national political psyche" and the "public mind," but has also ventured on omnibus explanation. He can catalogue, for example, the "psychic [and 'desperate'] needs of the American people." He informs us that Americans are "fascinated by unsavory creatures," that they "have respect for power," that Americans have "nativist instincts" and that the admiration for the "hero of iron" is "rooted deep . . . in the American value system" (pp. 64, 65, 69, 71). We are told, furthermore, that Americans were "titillated" by Mussolini's amorous exploits because an entire generation had been "weaned, as it was, on Freud and gin" (p. 63). Diggins's book is simply fat with such "information."

In effect, there is little in Diggins's book that could pass as warranted social science generalizations or cognitively supported explanation. At its best (in chapter 17), the author reviews with insight and considerable critical skill, the major efforts at interpreting Fascism—and finds them wanting. Which suggests that Diggins might have made a better case if he had insisted that Americans have been, and are, much confused about what to make of Fascism.

The best that could be said, on the basis of what Diggins has told us, is that "American opinion was hopelessly complex and painfully divided" (p. 322; cf. pp. 184–185, 199, 201–202, 205–207, 228, 233)—which seems plausible if unexciting. Diggins provides a convenient repository of vaguely characterized, but suggestive, descriptive claims about various sorts of published opinions attributable to various ill-defined populations. He has reviewed a wide range of literature and given *his* judgments about its content. Beyond that the book cannot pretend to tell us anything about the "American political psyche" or the "heart of American political values" that reasonably sophisticated folk-wisdom could not tell us with as much warrant. Still less can Diggins's book tell us anything about the "essence," the "reality," or the "nature" of Fascism.

Among the volumes devoted to treatments of special aspects of Fascism,[4] Roland Sarti's *Fascism and the Industrial Leadership in Italy* is quite another matter. Sarti's book is an effort to explore, descriptively and without pretension, the relationship between the Fascist political elite and the industrial leadership of Italy (defined in terms of the relatively small group of leading personalities who held office or were active in Italy's two most influential business pressure groups, the *Confederazione Generale dell'Industria Italiana* and the *Associazione frale Società Italiane per Azioni*). By comparing the convictions of the leaders of *Confindustria* and the *Associazione,* as represented in their published writings and private correspondence,

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[4] There are any number of books devoted to special aspects of the Fascist Regime. Among the best are F. W. Deakin's *The Brutal Friendship* (New York: Harper & Row, 1962), which deals with the final years of the Fascist Regime, and Alberto Aquarone's *L'organizzazione dello stato totalitario* (Turin: Einaudi, 1965) which deals with the organizational infrastructure of the "totalitarian" state. Several reasonably good books on Fascist diplomacy have been written from a variety of points of view. That of Giorgio Rumi, *Alle origini della politica estera fascista* (Bari: Laterza, 1968) deals with the period from 1918 through 1923 and exploits archival material hitherto inaccessible. Luigi Villari's *Italian Foreign Policy under Mussolini* (New York: Devin-Adair, 1956) should be read, if only because it is philofascistic. Giorgio Perich's *Mussolini nei Balcani* (Milan: Longanesi, 1966) and Francesco Jacomoni di San Savino's *La politica dell'Italia in Albania* (Rocca San Casciano: Cappelli, 1965) are specialized histories of Fascist relations with the Balkan countries. Carlo Silvestri's *Mussolini, Graziani e l'antifascismo* (Milan: Longanesi, 1949) is a specialized history of various aspects of Mussolini's Social Republic. It generated a storm of controversy at its publication and merits reading. There are a number of histories and documentaries devoted to the relationship between Fascism and the Italian press. Among the more interesting is the volume by Alfredo Signoretti, *"La Stampa" in camicia nera* (Rome: Volpe, 1968) and the anthology edited by Oreste Del Buono, *Eia, Eia, Eia, Alalà! La stampa italiana sotto il fascismo* (Milan: Feltrinella, 1971).

Sarti provides considerable insight into their political and economic aspirations. By comparing their overt and parochial intentions with manifest Fascist practice we can begin to make some substantive judgment about the relationship between a reasonably well-characterized body of men that constituted a significant part of the industrial leadership of Italy and the political behavior of Fascist Italy. The book provides a body of reasonably hard data against which the merit of generalizations entertained by any number of social scientists can be effectively gauged.

The range of Sarti's interests permits him to confine his claims to manageable dimensions. Some of his broader judgments, however, are sufficiently important or arresting to have merited more elaborate development and more systematic support. He ascribes, for example, Italy's postwar "economic miracle," at least in considerable part, to policies undertaken by the Fascist Regime (pp. 122–3, something suggested by Ludovico Garruccio[5] as well). Sarti also insists that through the agencies of the Fascist *Istituto per la Riconstruzione Industriale* "the government [exercised] a control over the economy that was unequalled outside the Soviet Union" (p. 124)—a judgment that merits more extended discussion. Moreover, some indication of the effects of overall Fascist economic policy might have been surveyed, since there is very little in English that assists even specialists in making an appraisal. But granting that it might have done more, Sarti's book is a model instance of competent contemporary historiography that recommends itself to anyone seriously interested in the Fascist phenomenon.

General Histories and the Understanding of Fascism

Accounts that are less specific and more broadly historical in character tend to be more self-consciously "theoretical" and more specifically "causal." F. L. Carsten's *The Rise of Fascism,* as a case in point and at its very beginning, provides what Carsten takes to be a partial catalogue of empirical factors that created the "preconditions for the development of ['fascist'] movements . . ." (pp. 9–10). One such factor, for example, was the prevalence of insecure lower middle classes, threatened by both the process of concentration in industry and trade on the one hand, and the growth, organization and restiveness of the working classes on the other. All fascist movements, in some sense, were to profit from such circumstances. Their membership was to be obtained by mobilizing the "uprooted," those whose "traditional ways of life" had been "eroded" (pp. 56, 10). The postwar crisis and economic dislocation, the insistent fear of communist revolution, and the weakness and ineffectiveness of parliamentary government all conjoined to produce fascist victory (p. 78). Aided and abetted by the connivance or the apathy of the government, the constabulary and the military, the fascists acceded to power.

Such generalizations constitute the explanatory core of Carsten's account of the rise of generic fascism. They are all stenographic and synoptic renderings of generalizations produced in the social science literature by practitioners like Robert Michels, Emil Lederer, and Heinrich Bennecke and were commonplace by the time Talcott Parsons wrote his brief account of the "sociological aspects" of fascist movements in the 'forties.[6] They have regularly appeared in discursive and historical treatments ever since.[7]

The difficulty with such generalizations, even when they issue from social science tracts, is that one is really at a loss to specify how they might be confirmed or disconfirmed. To say, for example, the "lower middle classes" have been "threatened" and rendered "insecure" by threats of revolution, or as a consequence of industrial developments, is intuitively plausible. Empirical evidence of felt insecurity could probably be collected without too much difficulty. But even granting that, one hasn't the faintest notion *how much* threat and *how much* insecurity is necessary to produce even a low-order probability concerning fascist recruitment successes. In order to be successful, "fascism" apparently requires a host of vaguely specified preconditions: a threatened lower middle class

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[5] Ludovico Garruccio, *L'industrializzazione tra nazionalismo e rivoluzione* (Bologna: Mulino, 1969), pp. 139–45.

[6] Cf. Robert Michels, *Sozialismus und Faschismus in Italien,* 2 volumes (Munich: Meyer & Jensen, 1925); Emil Lederer, *The State of the Masses* (New York: Norton, 1940); Heinrich Bennecke, *Wirtschaftliche Depression und politischer Radikalismus* (Vienna: Olzog, 1968); Talcott Parsons, "Some Sociological Aspects of Fascist Movements," *Essays in Sociological Theory,* revised edition (New York: Free Press, 1959).

[7] These kinds of formulations are found sprinkled through works of the following kind: Jose Ortega y Gasset, *The Revolt of the Masses* (New York: Norton, 1932); Hans Kohn, *Political Ideologies of the Twentieth Century* (New York: Harper, 1966); Angelo Tasca, *Nascita e avvento del fascismo* (Rome: La nuova Italia, 1950); Hannah Arendt, *The Origins of Totalitarianism* (New York: Harcourt, Brace, 1951); George D. H. Cole, *Socialism and Fascism,* 1931–1939 (New York: St. Martin's, 1960) and even Eric Hoffer, *The True Believer* (New York: Harper & Row, 1951).

(or classes), displaced masses, destabilized youth, protracted economic dislocation, a military catastrophe or a national humiliation of some significant magnitude, and the availability of leaders gifted with some special personality traits.

One doesn't have to be particularly clever to realize that such a catalogue of preconditions seems to identify the circumstances antecedent to almost *any* modern revolution. Moreover, neither threshold levels, indices, nor specific quantitative measures are suggested that might characterize any of these necessary or contingent conditions. Furthermore, we aren't given much of a clue about how inclusive or restrictive the category "lower middle class" is to be.

Historical accounts like Carsten's are best conceived as repositories of selectively collected and reasonably well attested factual data which might suggest possible empirical research. Each historian will collect, store and recount such data on the basis of his own often vaguely characterized sorting criteria. Fortunately, each historian has not only a task differing in range and focus from those chosen by other historians, but brings different sorting criteria to his enterprise. Social scientists can sometimes use the data provided as tests of reasonably specific empirical generalizations or as the basis of possible verificational studies. But to serve in such capacity an historical work must be reasonably specialized (like that of Sarti). Nonspecialized books like Carsten's can only serve such ends moderately well. Carsten's discussion of Italian Fascism, for example, is far too general to be mustered to such tasks. He is much less general, and consequently at his best, when he discusses National Socialism. Here, as distinct from his discussion of Italian Fascism, his touch is sure. He is dealing largely with primary source material.

Carsten's book can best be conceived as a pedagogical tool, a synoptic and interesting discussion of a broad and relatively shapeless topic. That Carsten imagined that such a book might have "explained how it was possible that [fascist] movements not only developed, but were able to become mass movements and to seize power" (p. 8) can be most charitably attributed to an excess of enthusiasm. The book is an interesting discussion of a wide variety of "fascist" movements, but it affords little in the way of serious *explanation*. Still less, unfortunately, does it provide a handle on why such movements should all be characterized as "fascist."

In his last chapter Carsten provides what he takes to be a criterial definition of "fascism." A "fascist" movement, for Carsten, is one which is (1) strongly nationalist, (2) anticommunist and anti-Marxist, (3) antiliberal and antidemocratic, (4) characterized by a hierarchically organized one-party state that is "strongly elitist in character," (5) possessed of an ideology that includes a "powerful myth," a "myth" that glorifies the past, and/or the "Leader" who is "venerated like a Saint by the faithful who could do no wrong and must not be criticized, who was God-given, and to whom superhuman qualities [are] attributed." The "Leader" is conceived to be possessed of a "personal dynamism and magnetism which could arouse vast crowds to frenzy and ecstasy" (p. 231). "Fascist" movements are further characterized, according to Carsten, by (6) a "cult of violence and 'action.'" They also (7) "appealed to all social groups" except those identified as "the profiteers, the parasites, the financial gangsters, the ruling cliques, the rapacious capitalists," and "the reactionary landowners" (p. 232).

One need not be particularly astute to recognize that such a criterial characterization describes not only "fascist" movements but also most "African Socialist" movements and most "revolutionary movements" in underdeveloped countries, as well as some self-characterized "Communist" regimes. Now that the Soviet Union has decided that Maoism is a "petty bourgeois" form of "anti-Marxist adventurism," it is difficult to know, for instance, how Carsten's characterization of "fascism" would exclude Maoism.

Theoretical Approaches to the Understanding of Fascism

Our more contemporary historians have become increasingly circumspect in claiming to "understand or explain" fascism. They have become, moreover, increasingly aware of the methodological obligations they assume in pursuing their craft. The books by Gilbert Allardyce and H. Roderick Kedward constitute evidence of that circumspection and that insistent sense of obligation. Allardyce, for example, is quick to admit that "few scholars are convinced that they fully understand [fascism]" (p. 1). Kedward similarly insists that "the history of fascism is still in its infancy" (p. v). Allardyce admits that historians use the generic term "fascism"—but he also recognizes that they have not agreed on how the term might be defined (p. 6). He then proceeds to arrest our attention with theoretical snippets from the interpretive works of R. Palme Dutt, Erich Fromm, Peter Drucker, Carl J. Friedrich, Hannah Arendt, Eugen Weber, Seymour Martin Lipset, John

Weiss, Barrington Moore, Ernst Nolte and Wolfgang Sauer. Not only has a reasonably precise definition of "fascism" eluded us—but we are provided with *eleven* explanatory theories, each promising purchase on "understanding." We have an embarrassment of riches. Kedward chooses to reduce the abundance of eleven candidate "theories" to three or four "approaches." Fortunately, there is a convenient overlap between the eleven "theories" of Allardyce and the three "approaches" of Kedward.

Allardyce reviews very briefly and competently the range of "classical" interpretations, despite his seemingly irrepressible disposition to find political motives behind every candidate interpretation. He does seem to emphasize unnecessarily the "moral dimensions" of "understanding" (as do Diggins and Carsten)—as though fascism were somehow more subject to moral assessment than, for example, Stalinism might be. It would be difficult to make a case for Mussolini's Fascism being more barbarous and more murderous than Stalinism—or Maoism or Castroism, for that matter. Furthermore, if as Allardyce suggests, National Socialism was an "aberrant" form of fascism (p. 5), then there is no reason to believe that the proper understanding of *even* generic fascism requires more "moral sensitivity" than the proper understanding of Stalinism, Maoism or Castroism. It would seem that we could proceed with our business with no more, nor no less, "moral sensitivity" in the case of Italian Fascism *or* generic Fascism than would be the case in our treatment of Stalinism, Maoism, Castroism, Nkrumahism or Nasserism. We can inspect the suggested interpretation of fascism armed with the same measure of detachment or involvement, the same intrinsic norms of our discipline, as we bring to the study of any complex political phenomenon.

Kedward's book, like Carsten's is a handy pedagogical aid that provides a brief account of a number of fascist movements. Contemporary students are not likely to know anything about fascism and Kedward's book is perhaps less freighted with errors than most. Which is not to say that it is afflicted with none. There are a few silly errors: on page 52 Kedward soberly tells his readers, for instance, that Hitler had brown eyes! Some of Kedward's judgments suggest that his treatment of Fascism is more than a little tendentious. We are told, for example, that "Mussolini's regime" failed "to bring industrial greatness to Italy" (p. 111). If we recognize the very narrow industrial base with which Italy entered the twentieth century, and the peninsula's almost total lack of the most basic essentials for industrial growth—fossil fuels and iron ore—Italy *did*, in fact, make significant strides toward industrial maturity during the Fascist period.[8] Every index of economic growth we have devised—total volume of output, industrial output, output per man, output per man-hour, and overall rates of capital formation—indicates that Fascist Italy went through sustained and substantial growth. Whether Fascism or any other system could *ever* bring "industrial greatness" to as implausible a place as Italy is a moot question. But it does seem to have produced sustained and significant rates of industrial and overall economic growth in Italy.

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[8] In this regard, cf. William Welk, *Fascist Economic Policy* (Cambridge: Harvard University, 1938); Antonio Benni, "Lo sviluppo industriale dell'Italia fascista," in *Lo Stato Mussoliniano*, ed. Tomaso Sillani (Rome: La Rassegna Italiana, 1930), updated as "The Industrial Development of Fascist Italy," in *What is Fascism and Why?*, ed. Tomaso Sillani (New York: Macmillan, 1931). The most readily available comparable aggregate statistics on Fascist economic development are to be found in Angus Maddison, *Economic Growth in the West* (New York: Twentieth Century Fund, 1964). There is no single comprehensive and competent account of Fascist economic policy and achievement. Of the many discursive treatments of Fascist economic policies, almost all are totally inadequate for anyone attempting to assess overall economic performance. Even the treatment by an internationally recognized economist, Paul Einzig (*The Economic Foundations of Fascism* [New York: Macmillan, 1933]), is loosely argued and almost totally innocent of any statistical analysis, and contemporary books like that of A. F. K. Organsky, *The Stages of Political Development* (New York: Knopf, 1965), devoted to the analysis of Fascism as an economic system, fail to cite any substantive data on industrial growth, diversification, or sectoral change. Henry Ashby Turner's recent essay on "Fascism and Modernization" (*World Politics*, 24 [July, 1972], 547–564), in which he attempts to make a case for an interpretation of "fascism" as an "antimodern utopianism," fails to include any data at all on rates of industrialization in Fascist Italy. Fascist publications provided an abundance of raw and presumably standardized data on the economic aspects of the Fascist Regime in publications like *Critica Fascista* and *Era Fascista*. An English edition of *Era Fascista* was published annually by the Fascist Confederation of Industrialists, but literally no effort has been made to sort out of that material a body of defensible statistical data that might provide insight into Fascist economic policy, its accomplishment or failure of accomplishment. To date we must remain content with general and fragmentary accounts like that to be found in Rosario Romeo, *Breve storia dell'industria italiana* (Rocca San Casciano: Cappelli, 1963) or tendentious and incomplete analyses of aspects of Fascist economic policy such as that found in Carl T. Schmidt's *Plough and the Sword* (New York: Columbia University, 1938). Brief accounts like that of S. Lombardini, "Italian Fascism and the Economy," in *The Nature of Fascism*, ed. S. J. Woolf (New York: Random House, 1969) and Shepard B. Clough, *The Economic History of Modern Italy* (New York: Columbia University, 1964), pp. 222–287, are suggestive, but hardly adequate. The most significant gap in the literature devoted to Mussolini's Fascism is to be found in the general area of economic analysis. Whatever evidence we *do* have seems clearly to indicate that Italy proceeded to economic maturity during the Fascist period. (In this regard cf. Garruccio, pp. 50–53, 140–144).

Whatever remaining flaws mar Kedward's account, they are by and large compensated for by his book's merits and by his concluding effort to provide a preliminary definition of "fascism" by rejecting the commonly conceived unilinear relationship between "fascism" on the "right" and "communism" on the "left"—and suggesting instead that the two "extremist" movements share a curvilinear relationship which makes them both "converge at many points" and renders them "indistinguishable" in some respects (p. 241).[9] Kedward does insist that "fascism" and "communism" are in some other senses "quite distinct," but he does not pursue the distinctions with any vigor.

Kedward's book, like that of Allardyce, is a good introduction to the undergraduate study of fascism. Both books—Kedward in interpretive summary and Allardyce with representative selections provide a brief introduction to the candidate "theories" of fascism that are now standard fare for aspiring social scientists.

Special Theories of Fascism: Neofreudianism

Fortunately, editors seem prepared to publish more substantial theoretical works for advanced students and professionals, and within the last few years we have seen the reissuance of two "classic" attempts at the interpretation of fascism as well as a new book which provides a contemporary version of a "Marxist" assessment of the phenomenon. The books by Wilhelm Reich and Peter Drucker originally appeared about thirty years ago.

The original German edition of Reich's *The Mass Psychology of Fascism* was incredibly bad when it first appeared, and irrespective of its continued popularity, it has not aged well. Today it reads like a caricature of speculative theorizing. Someone has even succeeded in making an unimaginably bad book even worse by preceding the text with a glossary of terms that provides the reader with stenographic definitions of such significant conceptual terms as "orgone energy," "vegetotherapy," and "biopathy." "Orgone energy," we are told (p. xxx), is the "Primordial Cosmic Energy . . . discovered by Wilhelm Reich between 1936 and 1940." "Vegetotherapy," in turn, is concerned with the "discovery of the muscular armor" which "binds up" "vegetative energies." "Vegetotherapy" concerns itself with the "concentration of atmospheric orgone energy" through the agency of an "orgone energy accumulator" (federal agents of the Food and Drug Administration please make note). "Biopathy," in turn, is a "disorder resulting from the disturbance of biological pulsation in the total organism" (p. xxix). Armed with this kind of conceptual apparatus we are presumably prepared to understand fascism.

"Fascism" we are surprised to discover, is nothing more than "the organized political expression of the structure of the average man's character" (p. xiii). "It is the mechanistic-mystical character of modern man," we are told, "that produces fascist parties . . ." (p. xiii). Since the *average* man's character produces fascism, it would seem that fascism should be a *universal phenomenon.* And that seems to be what Reich suggests. "[Fascism] could have developed in any country or nation. It is not a character trait that is confined specifically to the Germans or Italians. It is manifest in *every single individual* in the world" (p. 320, emphasis supplied).

The reason "every single individual in the world" is so seriously flawed is explained by "orgone biophysics." It seems that the "average man" and "every single individual in the world" has become "plasmatically rigid" by having "killed" his "genital function" (p. 341). "Man's biopathic character structure" is the product of "four to six thousand years of authoritarian mechanistic civilization" (p. 322). Man has become "biologically rigid," his "genital musculature" characteristically in a "spastic condition" (p. 151), because of the "ossification of the human plasma" (p. 344) brought about by preventing the individual from achieving "natural [sexual] gratification" (p. 31). The inhibition of the child's natural sexuality succeeds in the "damming up of sexual energy" (p. 89). These inhibitions began with the decline of the "matriarchal work democracy" which existed (by virtue of Reich's ethnological speculations) four or six thousand years ago—and with the advent of the "authoritarian patriarchal family" (pp. 138, 320, 322, 341). As a consequence, "all forms of fascistic, imperialistic, and dictatorial mysticism can be traced back to the mystical distortion of the vegetative sensations of life, a distortion that results from a patriarchal and authoritarian organization of the family and the state" (p. 136).

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[9] The literature that identifies the sustained similarities between "fascist" and "Marxist" political systems, particularly in terms of "totalitarianism," is abundant; cf. Carl Friedrich and Zbigniew Brzezinski, *Totalitarian Dictatorship and Autocracy* (New York: Praeger, 1956); Dante Germino. *The Italian Fascist Party in Power* (Minneapolis: University of Minnesota Press, 1959); Elie Halevy, *The Era of Tyrannies* (New York: Doubleday, 1965); David Ingersoll, *Communism, Fascism and Democracy* (Columbus, Ohio: Merrill, 1971); N. Kogan, "Discussion—Fascism and the Polity," in S. J. Woolf, *The Nature of Fascism;* Mihail Manoilescu, *Die einzige Partei* (Berlin: Stellberg, 1941); Mary Matossian, "Ideologies of Delayed Industrialization," in *Political Change in Underdeveloped Countries,* ed. John H. Kautsky (New York: Wiley, 1962); Ludwig von Mises, *Planned Chaos* (New York: Foundation for Economic Education, 1947); A. J. Gregor, "Classical Marxism and the Totalitarian Ethic," *Journal of Value Inquiry,* 2 (Spring, 1968) 58–72, and *The Fascist Persuasion in Radical Politics* (Princeton: Princeton University Press, forthcoming).

In order to "understand" fascism we must pay the exorbitant price of accepting Reich's bizarre speculations about the "damming up" of "cosmic orgone energy" that has afflicted mankind for "thousands of years" (cf. p. 357). Our inquiry began with an effort to understand the fascism that came to Germany and Italy—and now we discover that fascism is the product of disabilities suffered universally by mankind for "thousands of years." As though all that were not enough, Reich insists that "twentieth-century fascism"—for all its universality and millenarian roots—is "something completely new" (p. xxi). All of which cannot help but leave us somewhat confused.

Reich's book is only one of many written by psychoanalytic enthusiasts. It happens to be the worst. Peter Nathan's *Psychology of Fascism*[10] is a close runner-up. Hard behind that is R. Osborn's *The Psychology of Reaction.*[11] Erich Fromm's *Escape from Freedom*[12] (a fragment of which is contained in the Allardyce collection) is considerably better than most, but it is flawed by the same disabilities that seem to afflict this entire effort at interpretation.

Only if we grant that Reich or Fromm and their confreres have divined some special research method that permits them to generalize over whole populations on the basis of clinical studies undertaken with self-selected neurotics can we begin to generate propositions about the "human basis" of fascism. But even granting all that, we are left with serious puzzlements. According to Reich, "Marxist" revolutionaries in the Soviet Union can produce the perfect analogue of fascism (pp. 226, 236, 281, chap. 10). So it would seem that the sexual repression that originates in the "authoritarian family" constitutes the *necessary* and *sufficient* conditions for the advent of fascism. Fascism is a movement, then, that presumably springs from the conditions of the "authoritarian family"—a family which is, in the judgment of both Reich and Fromm, characteristically "middle class." Unhappily, all the serious research conducted after the Second World War failed to produce any compelling evidence of an unambiguous relationship between the middle class, the "authoritarian" family, and "fascist" or "protofascist" disposition.[13]

The speculative theorizing of psychoanalytically disposed authors cannot serve as even a partial interpretation of fascism. What such accounts do document, interestingly enough, are the shared similarities that overtly characterize "fascist" and "communist" political systems. Reich, Fromm, and Nathan all allude to the common property traits displayed by such authoritarian, hierarchically organized, one-party systems under the personalist dominance of a "charismatic" leader.

Special Theories of Fascism: Fascism as the Consequence of Moral Crisis

Peter Drucker, in his *End of Economic Man,* came to essentially the same insight. He concludes that "Soviet Russia is as fascist a state as Germany . . ." (p. 244). He arrives at that conclusion however, via a different theoretical itinerary.

Drucker's book is, of course, a far more substantial work than any in the psychoanalytic tradition. His chapter on fascist "noneconomic policy" still remains one of the best brief expositions of "fascist" and/or "totalitarian" economics: a policy of controlled consumption in the service of massive capital accumulation—an accumulation which provides the fixed capital assets of an expanding industrial system. Calling this perfectly comprehensible policy of rapid capital accumulation a "non-economic" strategy seems to be the consequence of Drucker's commitment to some special convictions about economic systems. For Drucker, an economic system should be geared to the *maximization* of *consumption,* the generation of *increased* and *immediate individual* welfare benefits. Anything else, it would seem, would be "non-economic."

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[10] Peter Nathan, *The Psychology of Fascism* (London: Faber, 1943).

[11] R. Osborn, *The Psychology of Reaction* (London: Gollancz, 1938).

[12] Erich Fromm, *Escape from Freedom* (New York: Holt, Rinehart, and Winston, 1941).

[13] After the Second World War a group of social scientists attempted to provide empirical evidence of the existence of, and the factors that produced, the "authoritarian personality"; cf. T. W. Adorno et al., *The Authoritarian Personality* (New York: Harper, 1950). The entire work was subjected to sustained and telling criticism in *Studies in the Scope and Method of "The Authoritarian Personality,"* ed. R. Christie and Marie Jahoda (New York: Free Press, 1954). Roger Brown provides a convenient review and criticism in his *Social Psychology* (New York: Free Press, 1965), particularly page 523. John Kirscht and Ronald C. Dillehay provide a sustained criticism in their *Dimensions of Authoritarianism: A Review of Research and Theory* (Lexington: University of Kentucky, 1967) particularly pp. 134f.

Because of this and a number of similar convictions, Drucker insists that "fascist" (and by implication, "socialist") systems do not "justify" themselves as "benefiting their subjects" (p. 14). Any political mobilization based on noneconomic satisfactions—"moral" rather than "material" rewards—would thus be *ipso facto,* "unjustified" and "unjustifiable."

Armed apparently with such a notion, Drucker proceeds to argue that "fascists" refused to "justify" their right to rule and that they appealed instead to the simple principle that "power is its own justification." Now, a veritable library of books is easily available that provides the fascist rationale for the exercise of specifically political power.[14] Drucker would be hard pressed to find the insistence that "power is its own justification" in any of them. Drucker may dismiss fascist (or for that matter "socialist") arguments as rationalizations, but he cannot pretend that fascists never attempted nor advanced an argument calculated to justify their political tenure (pp. 11–12). Worse than the insistence that fascists made no argument to license their right to exercise political power is Drucker's insistence that the "masses," because afflicted with "black despair," did not even demand that an argument be made.

Drucker insists that his book is calculated to deliver "an explanation and interpretation of totalitarianism which is valid and adequate" (p. xxvii). He also contends that he will accomplish that end by discussing political, social and economic matters. But what he conceives to be the *principal* cause of fascism becomes immediately evident. When we talk of "fundamental changes within the basis of social organization," he informs us, we "must" address ourselves to "a revolution of man's concept of his own nature, of the nature of his society, and of his own function and place in this society" (p. xxviii). Drucker is very specific: ". . . the true cause, the only possible cause, of a revolution is a fundamental and radical change in the order of values, especially in the most important sphere, man's conception of his own nature and his place in the universe and in society" (p. 10).

Drucker seems convinced that the ideas men entertain explain, in a direct anf forthright manner, their collective political behavior. Thus, he imagines that "it is the very essence of Europe that it conceives man as free and equal" (p. 50). Because of the traumatic insecurities that characterize the twentieth century, the "masses" have lost "faith" in the "two cornerstones" of the traditional order: "freedom" and "equality" (p. 58). As a consequence of that loss of faith, the "masses" abandoned themselves to fascist (and socialist) demagogues. Thus, in the last analysis, according to Drucker, fascism is the product of a general loss of faith in "freedom" and "equality."

At this point one can only suggest that not only has there not been an "essential European idea" of "freedom" and "equality"—there have been as many "European" notions of "freedom" and "equality" as there have been "European" political thinkers—but the notion that social and political change is the consequence of the taking up or abandoning of "ideas" by the "masses" is simplistic and implausible on its face.[15]

Drucker is astute enough to recognize that appeals to "national character" to explain social change constitute the "last resort of baffled historians unwilling to admit their inability to explain puzzling events . . . The national character of every modern people is so complex, seemingly so contradictory and so largely determined by intangibles, that almost anything can be read into it" (pp. 113f.). I would suggest that the attempt to attribute the possession of one or another collection of "motivating ideas" to the European "masses" is beset by the same disabilities. The psychological traits, the "value systems" and "ideas" of the "masses," are so complex, seemingly so contradictory, and so largely determined by intangibles, that almost anything can be read into them.

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[14] Fascist arguments are found in a wide variety of places. The most compelling are those of Giovanni Gentile, *The Genesis and Structure of Society* (Urbana: University of Illinois, 1960) and "Origini e dottrina del fascismo," in *Il fascismo,* ed. Costanzo Casucci (Bologna: Mulino, 1961); Sergio Panunzio, *Teoria generale dello stato fascista* (Rome: Cedam, 1939); Carlo Costamagna, *Dottrina del fascismo* (Turin: UTET, 1940), Antonio Canepa, *Sistema di dottrina del fascismo,* 3 volumes (Rome: Formiggini, 1937). There were efforts to vindicate Fascism's claim to political power as early as the organization of the Fascist movement; cf. Sergio Panunzio, *Stato nazionale e sindacati* (Milan: Imperia, 1924), *Lo stato fascista* (Rocca San Casciano: Cappelli, 1925); Balbino Giuliano, *L'esperienza politica dell'Italia* (Florence: Vallecchi, 1924); Dino Grandi, *Giovani* (Bologna: Zanichelli, 1941. [A collection of Grandi's essays from the period 1913 through 1920]).

[15] Drucker's notions are shared by Benedetto Croce; cf. "Chi è 'fascista'?" in Croce, *Pagine politiche* (Bari: Laterza, 1945), and "Il fascismo come pericolo mondiale," and "La libertà italiana nella libertà del mondo," in Croce, *Per la nuova vita dell'Italia* (Naples: Ricciardi, 1944). The same ideas resurface in a number of places where an explanation of Fascism is sought in terms of the "corruption of the Idea of Liberty"; cf. Luigi Einaudi, *Il buongoverno* (Bari: Laterza, 1954) and von Mises, *Planned Chaos.* The entire notion that "masses" act because their "ideas" or "conceptions" are "corrupted" is not only tendentious but scientifically primitive. In this regard see Reinhard Kuehnl's discussion of Ernst Nolte's *The Three Faces of Fascism* (New York: Holt, Rinehart & Winston, 1966) in "Problem einer Theorie ueber den Internationalen Faschismus," *Politische Vierteljahresschrift* 11 (November, 1970) 318–341.

Interestingly enough, Drucker admits that fascists insisted that they had "succeeded in discovering the secret of true freedom" (p. 80). Fascism also seems, according to Drucker, to have succeeded in going a "long way toward creating a genuine feeling of social equality among the lower classes" (p. 139). He also grants that fascism reduced unemployment, kept and expanded productive plant, and provided security where security had been wanting. All of which suggests that fascists, however much given to hyperbole they might have been, *did* advance arguments about "freedom" and *did* attempt to produce some kind of "equality" —and also succeeded in resolving *some* practical problems. Which suggests, in turn, that in order to explain the successes of fascist recruitment, one doesn't have to postulate a universal "mass despair" or an abandonment of the "European" ideas of "freedom" and "equality." People *may* have been attracted to fascism because its conception or "freedom" and "equality" made more sense, under the circumstances that obtained in Italy in 1922 and Germany in 1933, than the "standard European liberal" conceptions.[16] Now that a host of political systems in the world have opted for notions of "freedom" and "equality" manifestly different from those which Drucker (and others like Croce and von Mises) conceive to be the only ones admissible—it seems clear that one cannot confidently explain fascism in terms of the "abandonment" or "corruption" of the "idea" of "freedom" or "liberty." And if one does not have to argue that men choose fascism because they abandoned "freedom," then one is not driven to explain such "abandonment" by postulating a universal "black despair" that would drive the "masses" to seek "sorcerers," "magicians," and "miracles."

Whatever evidence we have from the fascist experience in Italy indicates that Fascist success was not the product exclusively or primarily of "mass despair" or the "corruption" of the "idea of liberty," but was at least in part a function of a conflict of very specific, proximate, and material group interests. The newly propertied small landholders of the Po valley, the large landholders, the technical and humanistic intelligentsia aspiring for employment in a restricted job market, threatened and insecure businessmen, war veterans, the constabulary and the military harrassed by anti-establishmentarian socialists, all found something in Fascism that appealed to their real, immediate, and material group interests.[17]

Fascism's fairly explicit political program— the maximization of national production through class collaboration of juridically recognized "productive categories"—was apparently no less rational than the political programs of its competitors. Finally, the maximization of national production would provide Italy and Italians with "freedom" against the "plutocratic imperialisms" of foreign powers—and the juridical recognition of productive categories would provide Italians with "equality."[18]

Special Theories of Fascism: Neo-Marxism

All of which sounds as though the interpretation offered by "Marxists" might be considerably closer to the mark.[19] Such interpretations of fascism have always turned on the "material interests" or organized and unorganized economic groups, classes or fragments of classes, and their complex interrelationships, rather than on the taking up or abandonment of "ideas" by the faceless "masses." Allardyce, in his collection of readings, provides a selection from R. Palme Dutt's "classic" Marxist interpretation of fascism, *Fascism and Social Revolution.*

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[16] For the Fascist treatment of the notions of "liberty" and "equality," cf. Gentile, *Genesis and Structure of Society;* Oscar Di Giamberardino, *L'individuo nell'etica fascista* (Florence: Vallecchi, 1940); Gerardo Pannese, *L'etica del fascismo* (Rome: Voce, 1942); G. Silvano Spinetti, *Fascismo e libertà* (Padua: CEDAM, 1941).

[17] The literature tracing the appeals of Fascism for various classes and segments of the population includes a wide variety of sources. For a discussion of Fascist appeals to large and small landholders, particularly in the Po valley, cf. Mario Missiroli, *Il fascismo e il colpo di stato dell'Ottobre, 1922* (Rocca San Casciano: Cappelli, 1966) and Giuseppe De Falco, "Il fascismo milizia di classe," in *Il fascismo e i partiti politici italiani,* ed. Renzo De Felice (Rocca San Casciano: Cappelli, 1966). For an account of Fascist appeals to declassed intellectuals and the technical and humanistic petty bourgeoisie, cf. Michels, *Sozialismus und faschismus,* pp. 253–323; Giovanni Ansaldo, "Il fascismo e la piccola borghesia technica," in Casucci's *Il fascismo;* and Luigi Salvatorelli, *Nazionalfascismo* (Turin: Gobetti, 1923). Renzo De Felice provides an extended account of Fascist appeals to business interests, veterans, the military and the constabulary; cf. *Mussolini il rivoluzionario,* particularly chaps. 12 and 14, and *Mussolini il fascista: la conquista del potere,* particularly chaps. 1, 3 and 4.

[18] Cf. Dino Grandi, "Le origini e la missione del fascismo," in De Felice, *Il fascismo e i partiti politici italiani;* and Ugo Spirito, "Il corporativismo come liberalismo assoluto e socialismo assoluto," in Casucci.

[19] The literature on the Marxist interpretation of Fascism is abundant. There is, of course, no *single* Marxist interpretation. One version is that of orthodox Marxism-Leninism standardized by the Third International which includes among its major expressions the work of Guilio Aquila, "Il fascismo Italiano," in De Felice, *Il fascismo e i partiti politici italiani;* Clara Zetkin, "Der Kampf gegen den Faschismus," in *Theorien ueber den Faschismus,* ed. Ernst Nolte (Berlin: Kiepenheuer & Witsch, 1967); Palmiro Togliatti, "A proposito del fascismo," in Casucci. R. Palme Dutt's *Fascism and Social Revolution* (New York: International Press, 1934) constitutes an effort to standardize and formalize

Actually, the work of Palme Dutt and the synoptic characterization of "Marxist" explanation given by Kedward represent only the "orthodox-Marxist" interpretation produced by the Third International in the mid-'thirties. Since that time even Soviet writers have themselves objected to such efforts as "stereotypic" and inadequate.[20] That such accounts were stereotypic and inadequate is about the *best* that can be said for them. Both Iring Fetscher and Renzo De Felice have weighted the "orthodox" interpretation and found it more than wanting.[21] But in point of fact there are *many* "Marxist" interpretations—and the "unorthodox" efforts are considerably better than those that have become "classic." Few accounts of the historic character of Italian Fascism written during the nineteen-'thirties, for example, are much better than that provided by Franz Borkenau.[22] But more than that, the contemporary "orthodox" Soviet interpretation is an account far different from that of the Third International.[23]

It is at this point that Reinhard Kuehnl's new book on fascism, *Formen buergerlicher Herrschaft* has special significance. For Kuehnl's book is a non-Soviet expression of the "Contemporary Soviet Standard Version" of fascism. Published about a year after the brief account given by Alexander Galkin[24] of the Soviet Academy of Sciences, Kuehnl's book is its faithful German rendering.

Where Palme Dutt could argue that fascism was nothing more than "the most typical expression of modern capitalist policy," Galkin insists that fascism was a "crisis regime" that could not be conceived as "necessary" to capitalism.[25] Kuehnl (p. 160) makes the same point.

For Palme Dutt fascism was the "instrument of capitalism," the "weapon of finance capital" —and was "maintained in funds and finally placed in power" by the "big-landlord, the industrialists and the big financiers"—to become the "terrorist dictatorship of big capital."[26] Galkin, in the more modern "Marxist" interpretation, conceives fascism to have been an *independent* mass movement which, because of the heterogeneity of interests that distinguished the groups that lent it qualified support, could act as an "independent force" with "definite autonomy." Using that autonomy, fascism went so far as to "infringe" on the "concrete interests" of "separate representatives and entire factions" of the "bourgeoisie."[27] Kuehnl makes the same points. Fascism was a "spontaneous" mass movement which exploited crisis conditions in Italy and Germany (p. 103). "Fascism did not serve capitalists as its lackey [*als blosser Buettel*]." Fascism was *not* capitalism's "in-

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the "orthodox Marxist-Leninist" account. John Cammett, "Communist Theories of Fascism, 1920–1935," *Science and Society,* 31 (Spring, 1967) 149–163, provides a reasonably competent survey of this version. S. M. Slobodskoi's *Storia del fascismo* (Palermo: Riuniti, 1962) is a trivial postwar restatement of the prewar version. Daniel Guerin, *Fascism and Big Business* (New York: Pioneer, 1939) is a prewar Trotskyist version of the "Marxist" thesis. Leon Trotsky and Antonio Gramsci left (for the purposes of analysis) only fragmentary comments on the fascist phenomenon; cf. Leon Trotsky, *Fascism: What it is—How to Fight it* (New York: Pioneer, 1944) and *The Struggle Against Fascism in Germany* (New York: Pathfinder, 1971); Antonio Gramsci, *Socialismo e fascismo: L'Ordine nuovo 1921–1922* (Turin: Einaudi, 1967). For the "non-orthodox Marxist" assessments of Fascism, the views of August Thalheimer ("Ueber den Faschismus"), Arthur Rosenberg ("Der Faschimus als Massenbewegung"), Herbert Marcuse ("Der Kampf gegen den Liberalismus in der totalitaeren Staatsauffassung") and Otto Bauer ("Der Faschismus"), are available in *Faschismus und Kapitalismus,* ed. Wolfgang Abendroth (Vienna: Europa Verlag, 1967). Ernesto Rossi's *Padroni del vapore e fascismo* (Bari: Laterza, 1966) is an "independent Marxist" account. Perhaps the most astonishing "Marxist" interpretation of Mussolini's Fascism is that provided recently by Mihaly Vajda of the Hungarian Academy of Sciences, "The Rise of Fascism in Italy and Germany," *Telos,* 12 (Summer, 1972), 32–6, in which Fascism is conceived to have been the "only progressive solution" to Italy's retarded industrial development!

[20] Boris R. Lopukhov, "Il problema del fascismo italiano negli scritti di autori sovietici," *Studi Storici,* 6 (1965) 240–255.

[21] Iring Fetscher, "Faschismus und Nationalsozialismus: Zur Kritik des Sowjetmarxistischen Faschismusbegriffs," *Politische Vierteljahresschrift,* 3 (March, 1962) 42–63; Renzo De Felice, *Le interpretazioni del fascismo* (Bari: Laterza, 1969).

[22] Franz Borkenau, "Zur Soziologie des Faschismus," in Nolte, *Theorien ueber den Faschismus.*

[23] Resolution zum Referat des Genossen Dimitrow, angenommen vom VII Kongress der Kommunistischem Internationale," in *Utopie und Mythos der Weltrevolution,* ed. Theo Pirker (Munich: Taschenbuch Verlag, 1964), pp. 226–229.

[24] Alexander Galkin, "Capitalist Society and Fascism," *Social Science, Soviet Academy of Science,* 2 (1970), 128–138.

[25] Palme Dutt, *Fascism and Social Revolution,* pp. 27, 69, 72; Galkin, "Capitalist Society and Fascism," pp. 128f.

[26] Palme Dutt, pp. 29, 155, 77, 79f., 89.

[27] Galkin, pp. 129f.

strument" (p. 104). The victory of fascism was the consequence of a coalition of convenience, an association of a spontaneous mass movement, supported, with an eye to their own interests, by nonfascist allies (p. 119). But because the interests of its capitalist allies were so "divergent," fascism retained a considerable latitude for autonomous action—not infrequently sacrificing the interests of some or all of its allies (pp. 122–123, 130, 134). Fascism in effect and in substance, was not the "dictatorship of finance-capital," nor was its "leader" finance-capitalism's executive instrument (p. 140).

While for Palme Dutt fascism was simply the capitalist state sanctioned by violence, for Galkin, fascism produced a *new state form* called "state monopoly capitalism" characterized not only by the relative autonomy of the fascist leadership, but by the fact that the state became "the guarantor of economic development" by providing the social overhead for the improvement and expansion of the economic infrastructure, the maintenance of necessary but noncompetitive industries, and in general "spurring up economic activity."[28] Not only that, but this new form of state pursued a "home policy" based on "social philanthropy." "The policy of social philanthropy was elevated to a state level."[29] Kuehnl doesn't seem prepared to go quite as far as Galkin, but he does admit that fascism did create a state structure that was "qualitatively new" (p. 118). One of its "functions" was to maintain high profit rates (cf. pp. 123–124) which is a left-handed way of saying that the fascist state maximized investment opportunities and effected considerable industrial expansion. Moreover Kuehnl admits, with Galkin, that fascism provided for a "degree of vertical social mobility" (p. 131).[30] Kuehnl also agrees that fascism set the "economy once more in motion" with large-scale public enterprises—perhaps a weak echo of Galkin's "social philanthropy" (p. 123).

What is more interesting about all this is not that the "Marxist" interpretation has significantly altered during the last thirty-five or forty years, nor that Kuehnl's interpretation is singularly unoriginal. What is instructive is how unpersuasive the efforts of both Galkin and Kuehnl are when they try to force these insights into a more orthodox Marxist framework. Both candidly admit that fascism is very threatening to the traditional elites, wresting from the hands of capitalists the "settlement of questions which for centuries" had been their prerogative. They both grant that the "bourgeoisie," in "handing over . . . power to the fascists" had, at the same time, to "subordinate" itself to the regime.[31] Even though the "capitalists" no longer can exercise their prerogatives in decision making and must subordinate themselves to the fascist regime, the regime remains for both Galkin and Kuehnl a "form of bourgeois rule!" Even though fascism "ran counter to the intrinsic conservatism of the bourgeoisie," fascism was, nonetheless, "bourgeois."[32] Even though Italian Fascism had so much "autonomy" that Paolo Alatri, one of contemporary Italy's Marxist intellectuals, could maintain that Mussolini was the "absolute master of the nation,"[33] Galkin and Kuehnl still insist that Italian Fascism somehow served the interests of the "big bourgeoisie." Even though Kuehnl carefully documents the use of political coercion and ultimately the use of terror against the "big bourgeoisie" (pp. 141, 147), he still insists that fascism served their "collective interests." With friends like the fascists, capitalism apparently doesn't need enemies.

The rest of Kuehnl's discussion is filled with commonplaces. Fascism was an ideology of "terror," it was characterized by an "ideology of community" and "authority." It emphasized the function of the "leader." It suppressed "opposition" ruthlessly. It infused the "masses" with its ideological substance. It gave a "military cast" to its invocations. It dichotomized the world into the "good" and the "bad." It was "nationalistic."

Had Kuehnl possessed a modicum of intellectual independence he would have noticed that none of these ideological traits distinguish fascism from contemporary "communist" regimes. Kuehnl thus leaves the analysis where it should begin. While the contemporary Marxist interpretation is significantly better than that produced by the Marxist-Leninist orthodoxy during the 'thirties, it is still lamentably short of providing a plausible interpretation of fascism.

For all that, fascism, even in this account, begins to look like what Italian Fascists regularly insisted Italian Fascism was—a mass mobilizing developmental dictatorship under single party auspices. Such a movement invokes whatever mobilizable population elements it finds available, and enters into *contingent* alliances with nonmovement allies to effect its purpose.

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[28] Palme Dutt, pp. 182, 193, 131.

[29] Galkin, p. 134.

[30] Galkin, p. 134.

[31] Galkin, p. 130.

[32] Galkin, p. 130.

[33] Paolo Alatri, *Le origini del fascismo* (Novara: Riuniti, 1963), p. 264.

Such a political system looks neither "capitalistic" nor "bourgeois." One could hardly even characterize it as "conservative" or "reactionary" for that matter, without considerable argument. In fact such a movement looks surprisingly like the mass mobilizing developmental dictatorships to which Mary Matossian and Robert Tucker allude.[34]

Conclusion

One really doesn't know what to make of all this.[35] Contemporary social science clearly has not provided us with a clear definition of fascism. All too many historians have not been able effectively to coin or support generalizations that hold their narratives together. More theoretically oriented social scientists have produced many "theories" of fascism that seem to be clearly defective in particulars, and only partially plausible in general. Some things, on the other hand, seem reasonably clear. Biographies are, in general, not very helpful in coming to understand complex political sequences—unless those biographies are more than that—unless they constitute detailed histories of the entire sequence. Most biographies are far too schematic, and in the case of Mussolini, so burdened by prejudgment that they can ill serve the professional social scientist.

Histories, in turn, tend to be very eclectic. Generalizations are borrowed from social science literature without critical assessment. Even Kuehnl, who is more the theoretician than the historian, uncritically invokes Reich's turgid account of "orgasm anxiety" in order to "explain" the "sado-masochistic" character of fascism (pp. 89, 152). In general, historians will invoke generalizations" about the "national political psyche," "mass behaviors," "insecurities," "despair," "nihilistic impulses," and "irrationality" with considerable abandon in the effort to provide a plausible account of obscure sequences.

Finally, the more specifically theoretical works, more pretentious in their efforts to "explain" and "understand," vary greatly in quality. Those devoted to a psychoanalytic "theory" of fascism are particularly insubstantial. Curiously enough, in the collection we have under our immediate inspection, the neo-Marxist interpretation of Kuehnl, suitably qualified, seems to be the most plausible. Of the snippets in the Allardyce collection the selection from Friedrich and Brzezinski (pp. 67–85)[36] is perhaps most suggestive, and that of Eugen Weber (pp. 95–109) perhaps the best summary introduction to fascism in English.

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[34] Matossian, "Ideologies of Delayed Industrialization"; Robert Tucker, "Towards a Comparative Politics of Movement-Regimes," *American Political Science Review*, 55 (June, 1961) 281–292.

[35] Cf. Leonard Shapiro, "What is Fascism?" *New York Review of Books*, 14 (February 12, 1970) 13–15; A. James Gregor, "Fascist Lexicon," *Trans-action*, 8 (May, 1971) 54–58; Gregor, "Totalitarian Age," *Trans-action*, 6 (October, 1969) 56–57.

[36] Friedrich and Brzezinski's criterial characterization of "totalitarianism" in *Totalitarian Dictatorship and Autocracy* is not, as both Friedrich and Brzezinski clearly understood, an *explanatory theory* of either fascism or totalitarianism (pp. vii, 7). What they offered was a *syndromatic and criterial definition* of a class of political systems called "totalitarianism." The concept was to serve as a mnemonic repository of "reasonably well-established matters of fact" as well as pedagogical aid of some consequence (p. vii). That they permitted themselves to talk of their work in terms of "models" and a "theory" is unfortunate. It conveyed the impression that they were attempting to *explain* and *predict* (although they were careful to call their account a "*descriptive* theory"). Most of the criticisms of "totalitarianism" that have become so popular of late turn on this misapprehension. Alexander Groth ("The 'isms' in Totalitarianism," *American Political Science Review*, 58 [December, 1964] 888–901); William Ebenstein ("The Study of Totalitarianism," *World Politics*, 10 [January, 1958] 274–288); Herbert Spiro (Totalitarianism," in *International Encyclopedia of the Social Sciences*, ed. David Sills [Volume 16. New York: Macmillan, 1968]); Michael Curtis ("Retreat from Totalitarianism," in Carl Friedrich, Michael Curtis and Benjamin Barber, *Totalitarianism in Perspective: Three Views* [New York: Praeger, 1969]); Otto Stammer ("Aspekte der Totalitarismusforschung," in *Wege der Totalitarismusforschung*, ed. Bruno Seidel and Siegfried Jenkner [Darmstadt: Wissenschaftliche buchgesellschaft, 1968]), and Peter Christian Ludz ("Entwurf einer soziologischen Theorie totalitaerverfasster Gesellschaft," in Seidel and Jenkner) all concern themselves with the putative explanatory and predictive pretensions of "the theory of totalitarianism." There is, in fact, no "theory" of totalitarianism—and such criticism is misplaced (the discussion found in Frederic J. Fleron, Jr., "Soviet Area Studies and the Social Sciences," in *Soviet Studies*, 19 [January, 1968] 313–339 is about the best). Similarly, the more contemporary criticism of the notion of totalitarianism which conceives it as the product of the "ideological requirements" of the Cold War period (cf. Herbert Spiro and Benjamin Barber, "Counter-Ideological Uses of 'Totalitarianism,'" *Politics and Society*, 1 [November, 1970] 3–21, and Benjamin Barber, "Conceptual Foundations and Totalitarianism," in Friedrich et al.) is equally unpersuasive. The fact is that the concept "totalitarianism" i.e., the suggestion that fascist and revolutionary "Marxist" political movements shared sustained similarities, dates from at least 1922 (cf. for example, Rodolfo Mondolfo, "Il fascismo in Italia," in De Felice, *Il fascismo e i partiti politici italiani;* Panunzio, *Che cos'e il fascismo;* and Giuseppe Prezzolini, "Ideologia e sentimento," in De Felice, *Il fascismo e i partiti politici italiani*), and is hardly the consequence of Cold War ideological bias. The similarities were recognized throughout the 'thirties and 'forties by such notables as Trotsky (*The Revolution Betrayed* [New York: John Day, 1937]), Erich Fromm (*Escape from Freedom*), Bruno Rizzi (*Dove va l'U.R.S.S.?* [Milan: La prora, 1928]), as well as by Fascists themselves (cf. Agostino Nasti, "L'Italia, il bolcevismo, la Russia," *Critica fascista*, 15 [March, 1937] 162–163; Tomaso Napolitano, "Il 'fascismo' di Stalin ovvero l'U.R.S.S. e noi," *Critica fascista*, 15 [October, 1937] 396–398; Berto Ricci, "Il 'fascismo' di Stalin," *Critica fascista*, 15 [July, 1937]

Efforts to synthesize the surviving elements of "classical" and more contemporary theories of fascism have made their appearance in the last few years. Some of the more interesting have grown directly out of W. W. Rostow's "stages of economic growth" thesis.[37] They include at least the works of Barrington Moore and A. F. K. Organsky.[38] The best of them all is perhaps that of Ludovico Garruccio,[39] but we still find ourselves without a viable and comprehensive theory of fascism. Whatever "understanding" we enjoy is partial—and must be painfully teased out of an inordinate amount of literature.

317–319. Certainly the conception of totalitarianism was not the *product* of the Cold War period. Its *popularity* might well have been. But that information helps us very little in assessing its cognitive merits.

[37] Walt W. Rostow, *Politics and the Stages of Growth* (New York: Cambridge University Press, 1971), *The Process of Economic Growth,* second ed. (New York: Norton, 1962), *The Stages of Economic Growth: A Non-Communist Manifesto* (New York: Cambridge University Press, 1960).

[38] Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon, 1966); Organsky, *The Stages of Political Development;* Organsky, "Fascism and Modernization," in Woolf, *The Nature of Fascism.*

[39] Garruccio, *L'industrializzazione tra nazionalismo e rivoluzione;* Garruccio, "Le tre età del fascismo," *Il mulino,* 213 (January-February, 1971) 53–73.

BOOK REVIEWS

Choice and the Politics of Allocation, a Developmental Theory. By David E. Apter. (New Haven and London: Yale University Press, 1971. Pp. 212. $7.95.)

This slender volume, which won the Woodrow Wilson prize in 1972, is vintage Apter: original and suggestive; often dense and opaque; sometimes unnecessarily abstract and polysyllabic; and largely barren of concrete illustrative material. To summarize the volume is impossible. Let me, however, provide a few hints for the casual reader. First read the last chapter, then the penultimate chapter, then dip into the rest, especially the summaries and particularly Chapter 1, as time and inclination permit. More serious students, who should omit nothing, might still profit by beginning in this fashion. In spite of this advice, and at the risk of verging on Apterian paradox, it may be suggested that the book needs not a reviewer's summary, but the author's elaboration. Perhaps Apter's response would be that this book must be considered in the context of his earlier work, and that it is at once a distillation of the essence of that work and a further development of it.

The volume deals with both modernizing (especially late period) and industrialized societies. Both should be concerned with choice; the former with its expansion, the latter with its equitable allocation. As modernization proceeds, the need for coercion increases (even while choice is being expanded); increased coercion means that less information will be available to decision makers; yet an industrialized society requires a maximum of information. How then can the transition be effectuated? Not easily. Political instability is inevitable; violence likely. The path toward a reconciliation system will be marked by embourgeoisement, bureaucratization, radicalization, and, frequently, revolution.

For each of his two types of society, Apter proposes a model constitution, designed not so much to be adhered to in detail as to suggest the directions by which constitution makers should be guided. The "development constitution" stresses efficiency rather than equity. To expand freedom, order must be imposed, even at the risk of considerable "unfreedom." Great reliance is placed upon corporate structures of power. They would form the basis for representation in the populist lower house. Stratification groups, segmentary groups, fused groups, differentiated groups, and functional status groups all would find place here. An imporant role would also be played by four "development commissions": "fiscal and economic," "education and manpower," "welfare and social services," and "security and justice." Representatives of these technical planning commissions would comprise an upper house of the legislature. One thinks, of course, of guild socialism and of all the problems to which critics have pointed in that system. Here they would be further complicated by the problem of relating the elected lower house to the nonelective upper house.

The "equity constitution," prescribed for the modern industrialized state, would be designed to aim at equity and the minimization of coercion. It should be democratic, decentralized, and highly participatory. In addition to a lower house that in this case would be constituted along rather traditional lines, the state would have an upper house composed of representatives of sociological and educational elites to serve as some sort of counterpoise to the "populist and interest elites" represented in the lower house. How the members of this body would be chosen, to whom they would be responsible, what would be their power base—these questions are left unanswered. The most important change from more conventional forms of government, we are told,

> involves the role of government in the information system. The Civil Service would be enlarged so that it might undertake information screening, processing, and evaluation. The lines tracing the information system would be punctuated by committees on consultation, including resolution committees and joint *ad hoc* committees.. Extraordinary sessions of both houses would also be possible, as well as hearings in which members of the planning commissions could present proposals (p. 185).

(Much of this, and perhaps even more of the "development constitution" is reminiscent of Sidney Webb's *Constitution for a Socialist Commonwealth of Great Britain.*)

In addition, the "equity constitution" would include a "political" high court, "whose task would be reconciliation of the normative dialectic" (p. 185). This court would be composed of planners, economists, sociologists, and other socioeconomic technocrats, rather than lawyers. Although its powers are not specified, its purpose would be the promotion of social justice. How this court would be related to the equity house is nowhere elucidated. Nor does the author attempt to explain how a "court" so constituted, and lacking the guidance and support and legitimacy supplied by a written constitution, a body of precedents, and the mystique of the law could hope to reconcile society's conflicting norms and make judgments that would stick.

Apter's normative position is that of a liberal socialist, although he prefers to consider himself an adherent to "social liberalism." The system he envisages would involve a mixture of public and private ownership "in which many sources of interest mediated through diverse forms of access to participation will be translated into a stream of messages, the decoding of which will define policy needs and obligations, the objects of decision-making" (p. 202). The system as a whole, and planning for it, would be highly decentralized. It is recognized that this goal would not be easily attained, but no hints for its accomplishment are forthcoming. Indeed Apter states at one point that "because of the large number of decisional units working more or less at cross-purposes, it may be difficult or impossible for decisions to be made. The sheer number of representative bodies may confuse the political process." (p. 177).

Indeed Apter is not unaware of many of the difficulties his system would confront, as the following passage indicates;

> The greater the degree of development, the wider the range of choice. The wider the range of choice in a system, the greater the degree of normative and structural imbalance, the greater the likelihood of alterations in the equity-allocation relationship and the legitimate authority of government. The greater the disruption in the equity allocation relationship and the reduction of the government's legitimate authority, the weaker the flow of information or the greater the need to apply coercion, the ultimate result being greater uncertainty. The way each political system responds to uncertainty, reaches its ceiling and alters, is our present concern (p. 141).

A final word. Apter's concern with the technical problems of government in dynamic societies, a concern that leads him into questions about the processing of information and the application of coercion and the relation between these two things, is paralleled by his concern for social justice. To too great an extent, in this reviewer's opinion, they remain discrete concerns, not adequately related in an integrated whole. The attempts that are made to relate them, as in the case of the "political" court, serve rather to highlight the difficulties than to assuage doubts. Yet I am loath to close on a negative note. This book is not only hard reading; it both reflects hard thinking and stimulates hard thinking.

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The Legacy of the German Refugee Intellectuals. Edited by Robert Boyers. (New York: Schocken Books, 1972. Pp. 307. $10.00.)

This is the reprint of a special issue published by the Journal *Salmagundi* (Skidmore College) in 1969–70. The very title arouses apprehensions. Which among the German refugees were "intellectuals"? There is no "Who's Who in German Emigration." The National Reference Service is quoted as listing 7622 "professional and academic" German emigrants between 1933 and 1945 (p. 47). Were all of them intellectuals, or only political leftists of a special literary caliber? Second, is it at all proper for German refugees—most of the authors are—to write on the relevance of contributions made by their peers to the host countries? Should not that judgment rather be left to the intellectuals of those countries? Would it not be more appropriate for us to testify to the influence America had on our own work?

The fourteen intellectuals honored by individual articles include T. W. Adorno, Hannah Arendt, Ernst Bloch, Bertold Brecht, Hermann Broch, Erich Kahler, Felix Kaufmann, Otto Kirchheimer, Thomas Mann, Karl Mannheim, Herbert Marcuse, Henry Pachter, Max Wertheimer, and even Karl Kraus because he, although no emigrant, "constituted a significant member of the emigree generation." Many others are mentioned incidentally. Yet the selection admittedly remains highly arbitrary.

Nonetheless, under the umbrella of these reservations I still wish to do justice to the book's considerable merits. Henry Pachter's introductory paper is above all a noteworthy, if in parts challengeable study on the experience of being an exile. In contrast to America, France, where he had sought asylum first, never accepted refugees fully as members of her society, he says. To the French authorities the German Left was at least as dangerous as Hitler. Once only did he see a chance of complete commitment, when the popular front emerged in France and Spain. He never felt so close to the masses, never experienced such a singular unity of cause and action. But even that did not bring about the integration he had sought.

In America, Pachter writes, democracy provides a favorable fighting arena for democrats; in Europe it often does this for its enemies. It is wrong to assume that refugees will always be liberals. He refers to Professors Hayek, Mises, Possony, and Strauss-Hupé for the opposite who would, I believe, call themselves liberals too. Americans see their own history as a success story, not as a story of conflict and tragedy. "American literature has produced no great tragedies and it does not deal with fate." Some refugees were proud to be citizens of this new Roman Empire, provided they could be "its Athenian teachers" (pp. 45, 46). Exile often destroys talent. There is nothing as inconsequential as emigree *querelles*. "We were writing about ghosts, writing for ghosts and gradually becoming ghosts" (pp. 15–19). Marxists and bohemians have in common that neither can bear the thought of being out of step; artists and exiles share contempt of the settled bourgeois style of life (pp. 24, 25). As regards success and influence, some emigrees like Einstein, Koehler, Panofsky, Schumpeter, Freud, or Ernst Cassirer, had been world famous before; others, like Tillich, Fromm, Mannheim, Adorno, Eduard Heimann, rose from local to world fame only through their work in exile (p. 47). But many failed to make the grade. Tucholsky, Hasenclever, Stefan Zweig, Walter Benjamin, Klaus Mann, Ernst Toller took their lives (p. 20). Hollywood's cemeteries, Marcuse has said, are the resting place of many Weimar personalities, including Jessner, the stage director, several great actors and actresses, Heinrich Mann, Franz Werfel, Bruno Frank, Lion Feuchtwanger (p. 51).

Pachter enumerates four fields in which the influx of European scholars has had great effect: logical empiricism, Protestant theology (with and without God), philosophy of history, and history of ideas (p. 48). American mathematics "was never the same after Professor Courant settled in New York University," and American musicology had almost not existed before "we" came. In the social sciences he goes so far as to compare the impact of the European sociologists to the invasion of classical studies by Greek scholars after 1453. In politics European scholars had more to learn here than they could teach (he calls this reviewer's *Political Theory* more American than David Easton's), but this very fact was one of the reasons that they have become so well equipped to explain America to Americans. Pachter celebrates the New School for Social Research as a place of experimentation and complete freedom for teachers and students. He feels tempted to say that "a place so European is possible only in America." In the 'forties and 'fifties students came to the School's Graduate Faculty because "nowhere else could one get genuine Max Weber, genuine Gestalt, genuine Phenomenology, or as close a reading of Plato as Leo Strauss offered" (pp. 34, 35). For his decision to stay in the United States after the War he gives a slighly escapist explanation. It was not, he writes, a sign of his predilection but rather that he "could be less involved here and need not identify with the burning issues of national politics to the extent one does in Europe" (p. 39).

Like Pachter, Professor George I. Mosse in a paper on the "Heritage of Socialist Humanism" seems to use the term "intellectuals" almost exclusively for political leftists of the nonviolent variety. Most left-wing refugees, he says, opposed the terrorist, totalitarian branch. He reaches the melancholy conclusion that the ideal of social humanism has been shown by recent history not to belong to the mainstream of human history which, he says, has stubbornly refused to follow the guidance of reason as the refugees have understood it. Their socialist humanism has remained a matter of European intellectuals.

Neither does existentialism owe its spread in America to the German refugees, for its reception had preceded their arrival. Thus concludes Professor Walter Kaufmann (Princeton) in his excellent essay on the origin and history of existentialism, a solid examination of the various ways the term has come to be used. He admits, though, that Tillich contributed to the fashion of authors presenting their own views as basically existentialist.

The papers on individual refugees are isolated studies with little interconnection. But most are worthy of praise in themselves as, for instance, Arnheim's fine essay on Max Wertheimer, a well-informed and ably written analysis of Gestalt philosophy in only seven pages, and those on Otto Kirchheimer by John Herz and on Felix Kaufmann by Reuben Abel. The following may be singled out for more expanded remarks.

"Thomas Mann and America" by Professor Hatfield (Harvard) and "Bertold Brecht and America" by Iring Fetcher (Frankfurt), while failing to say a single word on the influence these two men had on America, disclose remarkable parallels in their relations to America. Both had originally thought highly of the U.S. Both became increasingly critical, Mann after Roosevelt's death, Brecht much earlier, after his conversion to Marxism. Roosevelt appeared to Mann as a mythical figure, "almost a god," whose very shrewdness was a virtue since he used it towards meritorious goals. *Joseph in Egypt* is modeled in part after Roosevelt. In a sense, "Mann was standing before Pharao when he visited Roosevelt" (pp. 180, 182). Mann's withdrawal was motivated in part by his growing tendency to mistrust ideas of equality enforced from below. His own ideal was a democracy guided from above, with liberty not suppressed by equalitarian trends. Bert Brecht, by contrast, felt alienated by the "ways and tricks" which served to withhold equality. But he kept being thrilled by the large views, the generosity and urbanity of America (pp. 250, 257). Also, Marx had taught him to differentiate between the people and the social roles they are forced to play in the capitalistic system. "Inconsequential goodness" (*folgenlose Guete*) was no way out. "See to it that when you leave the world, not only had you been good, but you leave a good world". Brecht wrote in *St. Joan of the Stockyards*. If children are to learn how to adjust to society, one should ask first of all: to what kind of society (pp. 263, 266).

Karl Mannheim, who as a youngster emigrated from Hungary to Germany and then, in 1933, on to England, where he died as early as 1947, undoubtedly exercised great influence at home and abroad. But again, Professor Edward Sagarin and his partner Robert J. Kelly say little thereupon. They follow the development of Mannheim's ideas from the early stage when he held that the scholar's dependence on the epoch in which he lives and the circumstances under which he works makes it impossible for him to find absolute truth, to the later stage when he wrote that intellectuals might be able to reach a "detached perspective." He did not exclude the existence of truth as an absolute; he merely questioned the ability to see the truth (p. 279). The authors censure Mannheim's failure to take account of the fact that he himself was committed to an ideology—untotalitarian and democratic in character. They see the solution of this paradox in the postulate that the intellectual's commitment to scientific analysis must take precedence over his commitment to an ideology (p. 283), a point Eduard Spranger and this reviewer too have emphasized (see my *Political Theory*, pp. 298 ff. and 62).

Hermann Broch is praised by Erich Kahler as a great innovator for his use of different narrative styles in describing the various phases of the great crisis of our civilization. Kahler calls Broch's main work, *Der Tod des Vergil*, "one of the greatest though most difficult works of world literature . . . in its entirety unprecedented and original" (p. 187). It describes the last eighteen hours of Vergil when he comes to realize that in the pursuit of artistic perfection he has missed "the principal duty of the human being, the care of the good of his fellowmen" (p. 189). He even wants to destroy his *Aeneid*, but his friend, Emperor Augustus, wrests the work from him. Georg Steiner adds to this paper a note expressing his high esteem for Erich Kahler.

Ernst Bloch, the philosopher, is presented by Juergen Habermas (Frankfurt), as a "Marxist romantic" in an article which first appeared in Germany in 1960 and is here published in translation. Bloch's chief work, *Das Prinzip Hoffnung*, was written in the U.S., finished in East Germany and published in West Germany. To Bloch hunger rather than libido is the fundamental drive. Hope is man's most indispensable, most redeeming gift. Even the most evanescent wish-thoughts are elements of a great dream of the future. To a question I once asked him about how he would like to see the essence of his teaching described in a pocket-sized encyclopedia, Bloch replied: "S is not yet P," which was to mean that man, the subject, is not yet the predicate of what he potentially is. Habermas—although not immune to criticism of his own style of writing—blames Bloch's mannerism, pleonastic passages, metaphors "reminiscent of Boecklin's paintings," for the handicaps delaying his reception. "There are traces of the *Wandervogel*'s juvenile sociology even in the concept of hope" (p. 297).

Herbert Marcuse is dealt with only tangentially by Professor Anthony Wilden (San Diego), in a long paper—highly complex, in parts incomprehensible—which compares certain points in Marcuse's theories with those of Freud. Wilden plans a longer treatment elsewhere.

Professor Hannah Arendt is the only woman honored by a special article, ably written by George McKenna (City College), who is planning a book on her. She has always been notable for her impassioned engagement in current political issues as well as for classical erudition. Her concept of politics is based on the Athenian-Periclean paradigm, interpreted by her as a combination of action and speech. Action without speech is meaningless, speech without action is arid. That men can group together in order to produce change by word and action distinguishes them from animals. Only man can defy nature by starting something new. No man can be considered free unless he participates in the political life, not only once every few years by voting, but also in between. But action must be recorded in order to be remembered. No value relativism should stay the scholar from expressing passionate concern when describing concentration camps. Right, although I would say the scholar's exact description may be more telling than his emotional outburst.

This synopsis is apt to illustrate the variegated nature of the many legacies left by German refugee intellectuals and the lack of coherence among their messages, a universal crisis in present-day intellectualism. Just for this reason the book, although it does little to bridge the chasms that gape among the ideas of its heroes, deserves to be called important.

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The Costs of Accidents: A Legal and Economic Analysis. By Guido Calabresi (New Haven: Yale University Press, 1970. Pp. 340. $10.00, cloth; $4.50, paper.)

In a subfield which seems to thrive on the search for its particular scope and method, public law scholars should find *The Costs of Accidents* especially rewarding. There are two sets of reasons for this. First, this work by Guido Calabresi, a professor at the Yale Law School, provides a splendid introduction to and example of a new and rapidly growing form of sociological jurisprudence, the application of economic models to traditional problems of law. Second, his characterization and examination of accident law should convince even the most die-hard skeptic of the meaninglessness of the distinction between "private" and "public" law.

Abandoning doctrinal analysis for an examination of effects and purposes of the law, Calabresi views the law of accidents as a complicated court-administered means of governing behavior and resource allocation. "Since we are not committed to preserving life at any cost," he argues, "the question is the more complex one of how far we want to go to save lives and reduce accident costs" (p. 18). That is, rather than *one* narrow goal of accident law—the reduction of the number and severity of accidents—there are a number of other sometimes conflicting and competing goals. For example, there are also the goals of compensating the victims, spreading the losses to those most able to pay, reducing the costs of administering accident law, fostering socially desirable activities, and stimulating innovation of safety devices. Any complete system of accident law must consider them all. The common law with its separate doctrinal systems of fault, negligence, and strict liability has for the most part only indirectly and implicitly considered these questions. The aim of Calabresi's book is to face the choices more directly and provide guidance for legislative modification of these traditional systems.

This analysis fits comfortably within the tradition of sociological jurisprudence in that the author's purpose is to provide a framework for evaluation and a source of guidance for judicial and legislative policy makers. Like sociological jurisprudence and policy science generally, it is an instrumentalist analysis which goes beyond precedent, principle, and doctrine to examine goals, functions, and consequences of the applied doctrines. In this sense it is comparable to Bentham's examination of laws from the perspective for his felicific calculus and to Roscoe Pound's examination of law from an interest-balancing framework. By adopting an economic cost-benefit framework, however, Calabresi offers considerably more sophisticated style of analysis. Unlike the empirical or historical traditions of sociological jurisprudence, the economic framework he uses, with its rational choice/utility-maximizing assumptions, allows him to make *deductions* about behavior and the consequences of rules rather than remaining bound to limited inferences drawn from a limited set of data. For instance he can convincingly predict the probable reactions of the market place to proposals which shift liability from pedestrians to motorists. With this "theoretical freedom" and high level of generality he considers the problems of externalities and free riders, develops an explicitly systemic framework, and examines the frequently ignored problem of transaction costs—all standard features of economic analysis.

On the other hand, whatever evaluative strength there is in modern welfare economics is substantially weakened when he abandons the Pareto optimality criteria and confidently dives into interpersonal comparisons of utility without clearly specifying any other criteria. There is, therefore, nothing overwhelmingly convincing about the evaluative framework, and the result is essentially a cataloguing of the numerous "factors to be considered" in the various accident law policies and proposals. No keys to the identification of optimal combinations of them are offered, although to specify the set of boundaries convincingly is a feat in itself.

Turning to another feature of the book, by focusing on *consequences* of various alternative legal rules and their combinations, Calabresi demonstrates that tort law, and more specifically automobile accident law, clearly involves important collective choices. Various systems of liability and compensation represent important decisions about the exchange of lives and injuries for benefits, subsidization, income redistribution, and taxation—in short, all the problems associated with public regulation. For example, two of the problems Calabresi examines in great detail are the adequacies of the fault system as a general deterrent to accidents and its adequacies as a means of income redistribution—problems usually associated with criminal sanctions and taxation, not torts. That these types of policies have been formulated by courts (though increasingly by legislatures as well) relying exclusively on private law doctrines and not cost-benefit analysis (though some of the celebrated opinions of Holmes, Cardozo, and Hand approach the latter) does not negate the "publicness" of the results nor weaken the authoritativeness of the allocations.

It is an all-too-natural tendency for political scientists to focus on the "glamorous" battles and conflicts in the political arena. The danger in this focus, however, is that in complex, stable societies, the making of authoritative collective policies is likely to be fragmented, incremental and low in visibility, with one result being that political scientists fail to identify and concern themselves with many important types and sources of public policy. An emphasis on consequences rather than forum or fierceness of conflict would go a long way toward correcting this oversight. While such admonitions have repeatedly been made in efforts to direct attention to the impact of bureaucracy in modern societies, they have less often been made with respect to courts and common-law doctrines. Calabresi's book provides a compelling reason to consider the importance of courts and private law doctrine as important sources of public policies. No doubt the increasing interest in the use of tort law as a means of combating industrial pollution will also further sensitize social scientists to the public regulatory and control functions inherent in traditional "private" law mechanisms.

One last observation. While this book is explicitly evaluative, the public choice perspective does demonstrate the fruitfulness of the use of economic models for the characterization of legal problems. Consequently it also raises the possibility of the use of rational choice models for the development of *explanatory* theories of law and social control. To date only occasional and halting efforts have been made in this direction. Calabresi's book, along with Tullock's recent *Logic of the Law,* should, hopefully, stimulate additional interest in and begin to point the way for political scientists to undertake this far more interesting pursuit, the development of *explanatory* social theory.

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Moralphilosophie und Naturrecht bei Samuel Pufendorf. By Horst Denzer. (Munich: Verlag C. H. Beck, 1972. Pp. 405. No price given.)

How the mighty have fallen! At the time of his death in 1694, Samuel Pufendorf passed for (and perhaps actually was) the most important German political theorist and jurist of the 17th century; he was taken to be the synthesizer of Hobbes and Grotius, of state-sovereignty and "secularized" natural law. Locke, describing useful works on the "rise and extent of political power," called Pufendorf's chief work, *De Jure Naturae et Gentium,* "the best book of that kind"; Rousseau thought Pufendorf worthy of serious refutation in his *Discourse on Inequality;* and even Diderot's *Encyclopédie* followed *De Jure Naturae et Gentium* almost word for word in its articles on *états composés* and other questions of international relations theory. It is true that the two greatest German philosophers of the 17th and 18th centuries were not so impressed: Leibniz called Pufendorf "a bad lawyer and a worse philosopher," and Kant characterized him as a "miserable consoler" (mainly because of his "just war" doctrines). But in general Leonard Krieger is right in saying (*The Politics of Discretion: Pufendorf and the Acceptance of Natural Law* [University of Chicago Press, 1965], p. 2) that Pufendorf had "a contemporary influence that was immense and an ultimate impact that was minimal." Horst Denzer's new study, *Moralphilosophie und Naturrecht bei Samuel Pufendorf,* does not succeed in overturning that judgment.

The decline of Pufendorf's reputation is partly traceable to the difficulty involved in deciding whether his work is genuinely synthetic, or merely eclectic. If one reads the early chapters of his *De Officio Hominis et Civis*—an abridgment of *De Jure Naturae* made by Pufendorf for use in universities—one finds a definition of equity which is extracted from Aristotle's *Ethics,* a theory of sovereignty modeled on Hobbes's, a title borrowed from Cicero, and a doctrine of natural law ostensibly derived from Grotius (one must say "ostensibly" for reasons to be taken up shortly). Sometimes, however, one finds not only an eclecticism which does not quite qualify as synthesis, but actual inadequacy: in both *De Jure Naturae* and *De Officio*

Hominis, for example, Pufendorf claims (apparently with Hobbes) that the authority of law comes simply from "the power of the superior," but also (apparently with Grotius) that such superior power includes *both* "the strength to threaten some evil against those who resist him and just reasons [e.g., gratitude or a contract of obedience] why he can demand that the liberty of our will be limited at his pleasure." As long ago as 1706, Pufendorf's acute critic Leibniz observed in his *Opinion on the Principles of Pufendorf* that "if the source of law is the will of a superior and, inversely, a justifying cause of law is necessary in order to have a superior, a circle is created, than which none was ever more manifest." Krieger, indeed, makes much the same argument against Pufendorf in *The Politics of Discretion,* but traces the circularity of the argument to Pufendorf's "anxiety to adapt principles to practice," to be a *bourgeois* "trimmer" (p. 85), while Leibniz confined himself to charges of mere philosophical incapacity. But Denzer, in his *Moralphilosophie . . . bei Pufendorf,* touches on this point—indeed this *kind* of point—scarcely at all.

Despite his eclecticism and occasional inadequacy, however, Pufendorf is for the most part a solid, reasonable, and very thorough theorist who might well look better if not constantly compared with his greatest contemporaries. He is certainly far superior to every German political theorist of the 17th century with the exception of Leibniz, who was only a political thinker by fits and starts and never wrote a large-scale treatise as able as *De Jure Naturae et Gentium.* And Pufendorf could sometimes be quite eloquent, as some of the passages which Denzer cites show. Moreover, his distinction between *entia moralia* and *entia physica* anticipates Kant.

Like Pufendorf's work itself, Horst Denzer's *Moralphilosophie* is solid and careful and very thorough; it deals in detail with every concept that Pufendorf himself treated—including *entia physica, entia moralia,* natural law, the state of nature, the social contract, and sovereignty. If Denzer's book has a defect, it is that of being rather uncritical: to analysis and judgment of Pufendorf's ideas he prefers mere juxtaposition of those ideas with those of Pufendorf's "antecedents"; there is more historical comparison than philosophical evaluation. This is not surprising, given Denzer's stated aim of "placing" Pufendorf in the "field of intellectual and scientific history" (p. 22). But it is probably unjust to Pufendorf himself, who urged in his *Eris Scandica* that, "We should stop asking how new or old a doctrine is and investigate only how true or false it is."

One could wish that Denzer had heeded this excellent advice, for while his searches for the *Grundlagen* of Pufendorf's doctrines occasionally produce good results, as in the illuminating comparison of Pufendorf and Locke on toleration (pp. 216 ff.), he often falls into unsubtle characterizations of important theorists ("For Aristotle, as for Thomas [Aquinas], the community, the state, is the whole in politics, the individual is only a part; the whole comes before the part"). This is not incorrect, but it is something that anyone likely to be reading Pufendorf need not be told. And sometimes Denzer's method leads him to historical claims which are not just unsubtle or suspect, but actually perverse: Robinson Crusoe, one is told (p. 108) stands for "the rational individual as the starting-point in modern theories of the state."

A more serious problem, however, arises in Denzer's treatment of Grotius as the most important antecedent of Pufendorf. When he urges (p. 264) that Pufendorf's disagreements with Grotius are only "particular," never "fundamental," that they have to do only with the "better interpretation of this or that matter," he is on doubtful ground indeed. Grotius, of course, had become famous by arguing that "just as even God cannot cause that two times two should not make four, so He cannot cause that that which is intrinsically evil be not evil." In Grotian natural law, in short, the validity of moral laws is at least *comparable* to the necessary truths of mathematics. Now Pufendorf—as even Denzer admits (p. 53)—did not really share this view; indeed Denzer, paraphrasing Pufendorf's words, suggests that for him "there is no normative rule which is separate from the will of God. Every objective normative rule is in the end an effect of God's will." How, in view of such a high degree of quasi-Hobbesian voluntarism in Pufendorf's natural law, Denzer can characterize his disagreements with Grotius as "not fundamental," it is not easy to see. Surely the Cambridge Platonists, Leibniz, and Kant are the true heirs of Grotius' view that a moral rule cannot even be *conceived* as the mere effect of the will of a powerful being.

Ultimately Denzer's book must give way to Krieger's, which is better argued and much better written. But Denzer's work has three advantages which deserve mention: (1) his exposition of Pufendorf, if uncritical, is at least not hostile, as Krieger's sometimes is; (2) he quotes at length little-known passages on Pufendorf's methods and motives from the *Eris Scandica;*

(3) some particular parts—above all the one dealing with Weigel as the teacher of both Pufendorf and Leibniz—are good and informative. And Denzer's vast bibliography, which seems to list every edition of Pufendorf ever printed in any language, is certainly good to have.

PATRICK RILEY

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Bakunin on Anarchy: Selected Works by the Activist-Founder of World Anarchism. Edited, Translated, and with an Introduction by Sam Dolgoff. (New York: Alfred A. Knopf, 1972. Pp. xxvii, 405, vii. $10.00.)

Along with Herzen, Chernyshevsky, Lenin and Trotsky, Bakunin was one of the towering figures of the Russian revolutionary movement. Already during his own lifetime he acquired an international reputation as a militant revolutionary and theoretician of anarchism. The political history of his native country since 1917 and, in a larger sense, the development of authoritarian socialism in our century have made his ideas even more relevant than they were during his lifetime. No wonder that in recent years there has been a renaissance of interest in this romantic rebel: we can find echoes of his thought in the writings of Régis Debray, Frantz Fanon, Herbert Marcuse, and Eldridge Cleaver.

Bakunin developed a much more accurate vision of the nature and milieu of modern revolution than any of his contemporaries. Unlike Marx, Bakunin believed in the revolutionary potential of the landless peasantry and the *Lumpenproletariat* and recognized *déclassé* intellectuals as an important revolutionary force. Indeed, he envisaged the possibility of an alliance between the alienated intellectuals—the "dregs" of the middle class as Marx contemptuously called them—and the disinherited masses. From this perspective, the great revolutions of our century—in Russia, Spain, and China—were considerably closer to Bakunin's vision than to that of his great German adversary and antagonist in the International.

Bakunin, however, not only correctly anticipated the character and locale of modern revolution, but also foresaw its authoritarian consequences. As early as 1873, in his critique of the Marxist theory of the state, he predicted the emergence of a "new class" of political rulers, scientific experts, bureaucrats and state engineers who would once again enslave the people and nullify the initial gains of the revolution—all in the name of the "dictatorship of the proletariat." Bakunin's attack on the centralized bureaucratic state, his contempt for middle-class conventions, his insistence on "immediate revolution," his passionate argument for equality and liberty, his emphasis on the "instinctive" as opposed to the rational and scientific, his warning against the evils of a technocracy and the dangers inherent in subjecting human life to the rule of science—all these aspects of his thought make him a highly attractive and relevant thinker for our age.

The present volume, which was edited by Sam Dolgoff, a free-lance writer, labor union propagandist, and lecturer, is not only timely but also fills a major gap in English literature on socialism. Most of the writings included have never before been published in English or have appeared only in disjointed excerpts. Four of the twenty selections are from the pre-anarchist period of Bakunin's life. In addition to excerpts from such major works as *Federalism, Socialism, Anti-Theologism* (1867), *God and the State* (1871), and *Statism and Anarchy* (1873), Dolgoff's anthology contains a number of letters, programmatic statements, and short essays. The volume is arranged chronologically and includes a biographical sketch of Bakunin (1907) and an essay "On Building the New Social Order" (1874) by James Guillaume, a preface by Paul Avrich, a general introduction by the editor, and a useful bibliography.

In the case of Bakunin, the compilation of an anthology is a formidable task. He was not a systematic thinker and left a literary legacy consisting of a "patchwork of fragments," to use T. G. Masaryk's apt phrase. With the exception of *Statism and Anarchy*, Bakunin did not complete any of his major works, leaving even many short pieces unfinished. Furthermore, most of Bakunin's writings were polemical in nature, strongly reflecting the issues, political events, and circumstances of his time. For this reason it is impossible to read Bakunin's works meaningfully without a rather intimate knowledge of the historical context within which they were written. As in the case of any anthology, one may quarrel with the editor's choice of selections. In general, Dolgoff has exercised good judgment. He is less successful, however, in reconstructing the vital link between the ideas of Bakunin and his time. His introduction, notes, and amplifications, and Guillaume's biographical sketch do not always succeed in providing the reader with sufficient background information to understand and appreciate Bakunin's arguments and ideas.

While some will argue that there are limits to the benefits of neutral scholarship, the editor's introduction is too sympathetic. Dolgoff rightly stresses the libertarian principles expressed in

Bakunin's writings, but he does not really come to grips with the authoritarian implications of Bakunin's mania for secret organizations and conspiratorial revolutionary activity, his insistence on the necessity of an "invisible collective dictatorship" even after the revolution, or his confident prescription for "instant socialism." Moreover, Dolgoff too readily dismisses the arguments of those who have maintained that Bakunin was an authoritarian at heart and in some respects may be regarded as a forerunner of Lenin. In short, the interpretive mold into which Dolgoff seeks to force his subject, it seems to me, is too narrow, too rigid, and too simplistic to convey the full range, complexity, and ambivalence of Bakunin's spirit. In his preface, Professor Avrich is considerably closer to the mark.

Nevertheless, we are indebted to Sam Dolgoff for this anthology. The revival of interest in anarchism both in the United States and abroad and the continued vitality of the libertarian ideal of socialism as an alternative to the authoritarian perversions of that vision in our century suggest that the historical impact of Bakunin's ideas is far from having run its course.

ROLF H. W. THEEN

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Community and Purpose in America: An Analysis of American Political Theory. By Mason Drukman. (New York: McGraw-Hill, 1971. Pp. vii, 428. $9.95.)

Professor Drukman's traversal of the old ground of "American political theory" is inspired by a purpose, which is to say that this long book has a thesis. In the first chapter, the thesis is stated. America today is without a national purpose. We are a nation "riddled by mistrust and discontent," a nation "in genuine danger of falling into disarray." (On p. 3 Professor Drukman cites two novelists, Clancy Sigal and Norman Mailer, to support these claims.) At one time, America did have a national purpose: "liberty defined as economic individualism." Mr. Drukman's "central thesis" is that this national purpose (which began "to lose currency at the turn of the century" [p. 21]) has worked "to prevent the country from achieving a national sense of community" (p. 6). The failure of American political thought is "to treat seriously the idea of community" (p. 4). Thus "the customary mode of American theory has left us ill-equipped to think effectively about the situation before us *today*" (p. 21).

Of Drukman's two stated reasons for studying American political theory, understanding "the problems that confront us today" (p. vi) is clearly ascendant over the more theoretical activity of treating "it as real theory, material that . . . merits in its own right careful and thoughtful consideration" (p. 17). (Despite the reservations I will state, Mr. Drukman's own book deserves such consideration.) It is in terms of this ascendancy that his choice of American political thinkers can be explained.

There are chapters on Madison and John Adams, Paine and Jefferson, Hamilton, Emerson and Brownson, Calhoun and Fitzhugh, Sumner, Bellamy, Croly, Dewey, and Marcuse and Dahl. What seems to unite this group is that they are white males who thought about politics, hardly enough to define any sort of community. Drukman says that these men were chosen, with few exceptions, "from the mainstream itself." Some stream! Negro thinkers were excluded apparently because they were not "men who have mattered." "Impact of some noticeable significance constitutes the chief criterion for inclusion" (p. 23). "Impact" is obviously not confined to "political actors," although "the most renowned American theorists have in fact been themselves political actors . . ." (p. 15).

I think the author's criteria for inclusion and exclusion, the organization of his book, and the way he treats particular thinkers can be explained by his purpose (*supra*) and by his ideas about the limitations of "political actors" and his characterization of political philosophy.

Those "political actors" whom he does discuss in the first third of the book are, with the exception of Calhoun, founders: the men who shaped the institutions and categories of thought that enthroned "economic individualism" as the national purpose. We learn of the narrowness (and worse) of that purpose and therefore suspect the founders of myopia (if not worse). Being "political actors," these were "men whose ruminations could never stray from the object at hand" (p. 15). Thus, for example, Drukman criticizing Jefferson: "*reality*, with its inescapable set of limitations, incessantly crowded his style . . ." (p. 79); "[h]is ideal conceptions often succumbed to a mentality controlled by the dictates of reality" (p. 96).

If we are to get criticism, obviously we must abandon "political actors." Most of the remainder of the book deals with critics of "economic individualism" (except for the chapters on Sumner and Dahl, who are seen as overt or covert defenders of the old faith). But even these critics are limited; they do not achieve the realm of political philosophy. Although there is little apparent consistency in Drukman's use of the terms "political thought," "political theory," and "political philosophy," it is clear that political philosophy is at the top and seems minimally to require "a thoroughgoing critique of society, and an understandable set of suggestions for meaningful change" (p. 302). These critics do not achieve such heights. Although they may present "a forceful, sometimes radical, critique of important institutions and procedures," this is usually "followed by a tempering or narrowing of the argument so that in the end it does not break too drastically from accepted norms" (p. 24).

The author who comes closest to Drukman's ideal of political philosophy ("dialectical myth-making," "the heights of imaginative creativity" [p. 15]) is the novelist Edward Bellamy, whose "own account of the workings of his mind is very similar to what I have suggested is the creative process of the best political theorists" (p. 232).

Without claiming that our greatest statesmen ("political actors") were the American equals to Locke, Aristotle, or Rousseau, one can reasonably argue that they are better political teachers than is Edward Bellamy. By seeing political actors as "succumbing" to "the dictates of reality" and then identifying that reality with "economic individualism," Drukman unnecessarily narrows the horizons of our statesmen and underestimates their awareness of the problems involved in conceiving a nation in liberty and dedicating it to the proposition that all men are created equal. And by ignoring Lincoln, he does not see the possible nobility of such a dedication, of such a national purpose.

The American *political* tradition is far richer than the author would have us believe, and, in part, that wealth derives from the confrontation of minds with that disciplining reality which differentiates thought from fantasy. (The essays on American statesmen contained in *American Political Thought,* edited by Frisch and Stevens, demonstrate these contentions.)

Mr. Drukman's reliance on novelists ("imaginative creativity") to delineate our current plight and to suggest, in the case of Bellamy, the model of a political theorist, is less than compelling. The so-called "adversary culture" with its familiar certainties about current political and social reality may have been refuted by America's refusal to obey the call of the purveyor of those certainties to "come home."

RICHARD W. CROSBY

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Historians' Fallacies: Toward a Logic of Historical Thought. By David Hackett Fischer. (New York: Harper & Row, 1970. Pp. xxii, 338. $12.00.)

Professor Fischer's intentions in this book are clear and simple. His concerns are the ways in which historians set about their inquiries, the nature of their explanations of what they have observed, and the manner in which they present their 'arguments'—i.e., their solutions to problems, for "history is . . . a problem-solving discipline" (p. xv). His approach to these matters, however, is via the propensity of historians to fall into error, and in particular into methodological error. Most of his pages are devoted to a demolition job, to pulling down some of the wrong signs (categorized as an infinity of fallacies) which are found along the historian's path. To this catalogue of misdirections he adds "a few crude but hopefully more correct markers" to the true highroad which a properly professional historian should tread. These indicators are not less important because they bulk smaller than the list of historical fallacies.

Professor Fischer illustrates circumstantially the frailties of his profession. Erring colleagues are paraded from all ages and all lands. They are led by Tacitus, whose fallacy was moralistic; but he is followed by Hobbes, who fell into the fallacy of the one-dimensional man, and by Voltaire who succumbed to the fallacy of élitism. Others in the procession are, inevitably, Macaulay and Lytton Strachey; A. J. P. Taylor, "that Paganini of historical prose" (p. 35); and Namier, "a great historian and a gross Tory" (p. 69), qualities which perhaps cancel each other out. As for Professor Fischer's colleagues in his own country, they are compendiously described as "the methodological primitives who inhabit the history departments of the United States" (p. 52). Individuals including Arthur Schlesinger, Jr., and H. S. Commager are treated with varying degrees of severity; Robert W. Fogel, it is suggested, has led "his Cliometrical colleagues down the methodological rathole of the metaphysical question" (p. 18); and Gordon C. Bjork is arraigned for devising an index which is "a complicated thing, calculated from three other indices, all of which are incorrect" (p. 127).

In all of this Professor Fischer displays immensely wide reading and a gusto which is immeasurable. At the same time, his pace is so fast that he is sometimes in danger of falling into one of his own pitfalls. He demonstrates the fallacy of equivocation from an "extraordinary enthymeme" perpetrated by Byron Far-well; but are there not traces here of the fallacy of ambiguity, since how do we determine whether the word "enthymeme" is being used in its Ciceronian or in its Aristotelian sense? Further, his canvas is so vast that not all his critical judgments can be fully argued. The consequence may be something we might describe as the fallacy of the counterassertion. There is something of this in his arraignment of Bjork quoted above; and the "false and misleading" character (p. 83) of Eric Williams's interpretation of the history of Trinidad and Tobago is indicated rather than demonstrated. This fallacy has the further disadvantage in that it may prevent our asking whether there is anything or nothing of value in the treatment of this history. Perhaps, too, we may detect a fallacy of excessive minimization, as when Marxist-Leninist history is dismissed in 49 words (p. 174); or a fallacy of understatement, when a passage in the preface of Namier's *Structure of Politics* is translated as a refusal to name any sources since he cannot name them all. In fact Namier's very next paragraph lists the sources upon which he has relied most heavily, and his book is equipped with ample footnotes.

It would, however, be less than fair to leave Professor Fischer's essay on a negative note. It is in fact a good deal more than a rumbustious romp among historians' reputations. It has a serious and positive purpose. First, Professor Fischer is convinced of the utility of historical study and of the writing of history: for history can clarify the context of contemporary problems, indicate some possible lines of development, and refine the theoretical equipment we apply to questions of our own day. Second, precisely for these reasons it is vitally important that historians should perfect their own professional equipment: and his book has been inspired by his conviction that "the trouble with professional historians is that they are not professional enough" (p. 315). This is not to say that he claims too much for history. "The whole truth" is an impossible object; and "progress . . . depends upon a sense of the possible" (consequently he is at one with M. M. Postan in rejecting as impracticable the way of "metahistorians" like Toynbee). In its own way, nevertheless, history belongs with the useful arts, "adducing answers to specific questions" by rational and logical processes. If Professor Fischer has made us more self-conscious about the ways in which we set about that task, and still more if he has successfully blocked off some short-cuts which turn out to be blind alleys, his purpose will have been achieved and history will be the gainer.

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Capitalism and Social Theory: An Analysis of the Writings of Marx, Durkheim, and Max Weber. By Anthony Giddens. (Cambridge, England: Cambridge University Press, 1971. Pp. 261. $11.00.)

This book is an important contribution to a growing literature on some of the classics of the main sociological tradition. Instead of emphasizing the duality of the sociological tradition—as Alvin Gouldner does—with one major branch stemming from Marx and the other from Durkheim and Weber, Mr. Giddens stresses the common features of their diagnoses and conceptualizations. I think it is fair to say that the author emphasizes the centrality of Marx, although he does not take a specific position in favor of any of the three writers and declares his intention to be more expository than critical.

As an expository work, it has many merits. I found the author's discussion of Marx's early writings, especially about the genesis of the conceptions of alienation and exploitation, very illuminating. There is less which is relatively new in his expositions of Durkheim and Weber, but these are, on the whole, specific judgments, such as that stated in the preface that Durkheim was not primarily concerned with the problem of order but with a succession of orders, which is, I think, inadequately clarified. The two statements are not necessarily incompatible, but I would contest the first proposition that Durkheim was not in any serious sense concerned with *the* problems of order.

My main line of criticism, however, relates to a lack of clarity in the nature of the task which Mr. Giddens has undertaken. There seem to be three primary components in his discussion which should be more clearly distinguished from each other than they are. These are first, a history of sociological or social theory, including a discussion of the relevant empirical problems and data. The second is a sociology of knowledge which attempts to use analysis of the social situation in which the three authors lived in order to throw light on their statements and the genesis of their theoretical views. The third, related to the second, is the development of intellectual guidelines for courses of action, especially political action. This last problem was indeed a central one to all three of the writers. Marx in particular emphasized it in the famous phrase, "the unity of theory and praxis." Durkheim was very much concerned with social reform, and on a different level, so was Weber, though the last two, of course, were not revolutionists.

With respect to the relations of the first and second themes, there is no doubt that much "understanding" of an author's ideas can be gained from an analysis of the social, political, and cultural situation of his time. I do not contest for a moment the importance to Marx of what Giddens calls the "backwardness" of Germany, to Durkheim of the difficulties of France following the defeat of 1870, and to Weber of the nature and international position of Bismarck's and Kaiser Wilhelm's *Reich.*

My plea, however, would be for according considerably sharper attention to the development of theoretical conceptualization as such. The school of the historical analysis of social thought is similar to the approach in literary criticism so sharply criticized by Northrop Frye—an approach in which personalities and social situations of literary authors are given undue prominence relative to the actual content of their work. (Frye, "The Critical Path: An Essay on the Social Context of Literary Criticism," *Daedalus* [Spring 1970], pp. 268–342.) In my opinion, this tends to blur and relativize the significance of the social thought of major figures. Especially with respect to the relation of both Marx and Weber to the tradition of German thought, Giddens skates on the edge of a "historistic" point of view. This trend was strong in Marx himself, who considered classical—and *pari passim* this would include "neoclassical"—economic theory to be relevant only to the capitalist system. As for Weber, Giddens comes perilously close to Reinhard Bendix's view that Weber's theory was a helpful scaffolding in the construction of historical interpretations rather than theory in its own right (Reinhard Bendix and Guenther Roth, *Scholarship and Partisanship: Essays on Max Weber* [Berkeley, Calif.: University of California Press, 1971]; see also my review of Bendix and Roth in *Contemporary Sociology,* Vol. 1, no. 3 [May, 1972], pp. 200–203). Indeed, Giddens sometimes refers in a skeptical and almost derogatory tone to tendencies in current sociology to attempt to develop generalized theoretical schemes independent of particular historical circumstances. As theory comes to be carefully differentiated from the sociology of knowledge, these tendencies will be increasingly accentuated. Both Durkheim and Weber went much farther than Marx in upholding not only the desirability, but the feasibility of the development of generalized theory. Such theory built precisely on Marx's dialectical idea of the interplay between subjects and objects and the conception that social systems are essentially consequences of past human actions rather than elements of the world of nature in the physical-biological sense or consequences of a process of divine establishment.

Giddens makes a reasonably cogent case that the conflict between Marx on the one hand, and Durkheim and Weber on the other, over "materialism" is not adequately stated in terms of the inversion of Hegel's idealism. He is quite right that Weber was not a proponent of "determination" by ideas but a proponent of the extremely complex interdependence between ideas and other factors. A sharp difference between Marx and Weber lies in the fact that Marx tended to treat capitalistic society as a monolithic entity which included its political and ideological structure and to consider the political component of the capitalist firm as inseparable from the economic aspects of the firm's internal structure, that is, the capital-labor relation and the firm's involvement in the market nexus; Weber, on the other hand, developed a differentiated analysis of the complex modes of specified interdependence between "ideas" and economic and political structures. This analysis is related to his extensive comparative interests, whereas Marx's treatment of societies other than the capitalistic was, in Giddens's term, "sketchy." Similarly, on the methodological level, by distinguishing the "value free" status of the *cognitive standards* from the values operating in action, notably political action, Weber presented a much more differentiated view than the Marxian unity of theory and practice. Giddens also mentions Durkheim's significant stress on the differentiation in modern society between the state and what he called "society," which I would call the societal community.

The relatively low level of differentiatedness in Marx's theory is related to the sharpness of the dichotomy between capitalism in which men are alienated and that vaguely defined entity "the social character of human existence." Marx built this into a rigid dichotomy in which capitalism was held to be totally devoid of "sociality," if I may use such a term, whereas the ideal state of communism was alleged to be totally devoid of all the characterizing features of capitalism. By contrast, Durkheim and Weber refused this dichotomy. They saw modern society as involving, in different ways, elements of alienation and exploitation but they did not see it dissociated from what Toennies later conceptualized as *Gemeinschaft*. Both, like Marx, were acutely aware of strains and tensions and personally critical of many features of the society of their time, but both thought an evolutionary change more probable than the Marxian apocalyptic vision of violent revolution.

Both were much concerned with the status of labor in modern society. They laid stress on a development which has occurred since early capitalism and which, without violent revolution, has profoundly changed the state of that area of modern societies most closely related to economic production. I refer to the development of an *occupational* system in which the performance of labor has come to be institutionalized in a highly ramified system of occupational roles, only a minority of which fit the Marxian characterization of labor's status, especially as subject to stringent supervisory authority, to minute fragmentation, and to reduction to minimal levels of monetary remuneration. The institutionalization of managerial functions has developed in an occupational framework quite different from the Marxian "capitalist" stereoype. Another development is probably just as important in the long run, that of the modern professions, which play a crucial part in large-scale organizations and bridge the structural hiatus between private economic and governmental organization and the gap between both and the private, nonprofit sector. While I would be the last to argue that late industrial society is no longer characterized by class differences, those differences surely do not fit the pattern of the Marxian bourgeois-proletariat dichotomy.

Though Marx emphasized technology, he failed to foresee the importance not merely of an altogether new type of technology, namely science-based technology, but also the relation of the emergence of such technology to the development of the system of formal education, and especially higher education and research. Durkheim and Weber, partly because they lived in a later generation, were aware of the importance of these developments, although many contemporary sociologists would emphasize them much more than they did.

Mr. Giddens has written an important book which deserves serious attention among the interested professional groups. It will be a very convenient book for people who would like a secondary introduction to the authors he treats. I would advise the serious student, however, to pay close attention to the original texts. Giddens has made an important contribution to the interpretation of the significance of these major figures, but I think the last word is far from having been said.

Talcott Parsons

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A Source Book for the Study of Personality and Politics. Edited by Fred I. Greenstein and Michael Lerner. (Chicago: Markham Publishing Co., 1971. Pp. 572. $16.50.)

The study of personality and politics has two inherent sources of major interest as well as several significant problems. The interest derives largely from the importance of the individual political figures studied by investigators in this field and also from the potential significance of reliable findings about how the political process is influenced by particular persons. Thus, for example, personality analyses of Woodrow Wilson and Richard Nixon appeal by virtue of what they suggest about underlying determinants of decision making at higher levels of government. Similarly, investigations of the personality characteristics and political actions of liberals or conservatives are potentially important because they suggest that many political decisions flow from deeply rooted personality dispositions, rather than situational considerations.

The field also has limitations that have reduced its influence and raised doubts about those findings that have emerged. Among the factors limiting acceptance of those findings is the absence of psychological theories and research evidence that would provide a solid basis for guiding studies, conducting research, and evaluating findings. Psychoanalytic theory previously served this function for many investigators, but that theory no longer commands the loyalty it once did, and no other general theory of personality has replaced it among social scientists. Another problem involves the reliability of present psychometric instruments and procedures. This problem plagued such earlier studies in the area, e.g., the authoritarian personality study, and questions about response sets and defensiveness are far from resolved.

Despite these problems, the study of personality and politics has been growing rapidly in the past decade and has produced increasingly sophisticated research and theory. This work by Fred Greenstein and Michael Lerner provides an excellent indication of the current state of knowledge in this field. In their careful and scholarly sampling of the major research and theory in personality and politics, the editors present a portrait of a field that is striving for scientific respectability yet is bound to the present-day relatively limited armamentarium of psychology. The weapons in this armory include theories that can clarify murky uses yet are incapable of dealing with the complex interactions involved in political actions; individual studies that are well conceived, soundly executed and truly illuminating, but generally noncumulative. While the editors declare that the study of personality and politics is a lusty young infant, my reading left me with the feeling that the pregnancy is still in progress. I also had the impression that the interface between political science and psychology has serious limitations. At the interface with sociology, such concepts as orientation, style, and type are comfortably employed, and the complexities of personal exchange and interacting forces can be dealt with descriptively and without apology for lack of measurement data. In a certain sense, there is a sad but realistic recognition that present-day psychological theory and assessment is incapable of delivering the theory, concepts, and data that Greenstein and his coworkers so urgently require to attain their vision of a scientifically based field. Wisdom may require that the vision be examined to determine how it relates to the present, near future or long term future.

While it is possible to quibble with some of the choices and exclusions of the editors, there is little doubt that these twenty-eight articles represent significant research in the area and deal with the fundamental issues of the field. The articles are divided into four sections containing between four and ten articles each. The first section contains four papers dealing with general theoretical and methodological statements. The paper by Greenstein presents an initial overview of the field and, together with the paper by M. Brewster Smith, examines some of the major issues that underlie the complex interactions involved in personality and politics research. There follow two well-developed papers attacking simplistic efforts to reduce all political behavior to the psychological level (Bendix) and employ the concept of role without due appreciation of social forces (Levinson). The omissions of this section are as notable as the inclusions, for it contains no treatment of measurement problems, only passing treatment of research strategy, and little discussion of present limitations of this area of research and their implications.

The second section comprises five articles examining individual political actors including and applying depth psychological categories and procedures to the lives of Woodrow Wilson (Alexander George, Edwin Weinstein), Gandhi (Erik Erikson), the political attitudes of fathers and sons (Lane), and the resolution of decisional conflict (Janis). The hitherto unpubilshed essay by George provides a sound introduction to psychobiography, examines some of the deficiencies that occur in such works, and then considers how the approach can be productively employed by examining the personality of Woodrow Wilson. (This essay draws heavily on the booklength biography of Wilson written by George and J. L. George.)

Section Three presents ten papers dealing with personality analyses of types of political actors. These include noteworthy papers relating personality and political orientation (Di Palma and McClosky), overview and critique of the authoritarian type (Sanford, Shils), student protesters (Flacks), and presidential character and style (Barber). This section also includes classic papers on the functional approach to the study of attitudes (Katz), and the logic of political personality typologies (Lasswell). Section Four examines the effects of personality on the functioning of political institutions. Other articles present general conceptualizations of the individual and sociocultural system (Inkeles and Levinson), the impact of the leader's personality (Browning and Jacob), while others illustrate how survey (Converse and Dupeux), clinical (Osgood), and model-building (Greenstein) procedures can aid in the analysis of political actors. All in all, a nourishing and generally satisfying smorgasbord.

This work clearly attains its goal of providing a basic grounding on the subject for graduate students, advanced undergraduates, and scholars in other fields. While it complements Greenstein's book *Personality and Politics*, this collection can easily stand on its own in the quality and scope of its coverage. Although this collection reveals that the field is growing and improving, it appears presumptuous to call this work a "source" book. The word implies that the field has achieved an extensive body of knowledge and that the articles represent the major and definitive theory and research in the area. Both of these positions are open to dispute, and a less pretentious title would have underscored that this is indeed an excellent reader. I concluded that the study of personality and politics is to a considerable extent art as well as science and that pronouncements to the contrary do not alter that opinion. Conceptual analyses, theoretical maps, and clarifications are sharpening the issues, but the very complexities of the subject matter appear to be well beyond presently available methods of objective assessment. While the editors dwell on the progress that has been attained, they might have devoted more time to assessing the current state of the science/art, and the directions that are likely to prove productive, given present methodological implications.

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Cybernetics, Simulation, and Conflict Resolution: Third Annual Symposium of the American Society for Cybernetics. Edited by Douglas E. Knight, Huntington W. Curtis, and Lawrence J. Fogel. (New York: Spartan Books, 1971. Pp. 249. $10.00.)

Will the year 2000 mark the beginning of a thousand years of peace—perhaps directed by ultraintelligent machines (UIMs) with unlimited storage capacity? Douglas E. Knight, an editor of this compilation of Proceedings of the Third Annual Symposium of the American Society for Cybernetics, answers with a qualified yes, assuming that the year 2000 will see both improved simulation techniques and a more predictable (i.e., easily simulated) human race.

The symposium's task, in the words of Knight, was to explore whether the simulation laboratory could "offer a testing ground for developing possible alternatives to violence" (p. xv). The sixteen contributors undertake this exploration on four levels, which constitute the four sections of the book: I. A Problem: Campus Confrontation; II. Simulation of the Problem; III. International Conflict Analysis and Simulation; and IV. The Technology: Cybernetics in the Seventies and Beyond.

In his keynote address, Lawrence J. Fogel attributes modern man's hostility to increasing interpersonal competition forced upon him by greater and greater overlap in his goals—the result of increasing worldwide homogeneity. He sees cybernetics (the comparison of automatic control systems in man and machine) as the key to handling conflict, with the papers presented at the symposium spelling out the whys and hows.

In the first article, "Must We Change for Change's Sake?" author Preston Valien concludes that we really must. He sees as the culprit of academic unrest traditional, process-oriented education, in place of an emphasis on human needs. One wonders why Valien does not suggest that educational institutions teach change—the problems of impermanence and dislocation and how to induce change intelligently. David Gardner continues this line of thought in the next article by suggesting that the university break with past methods in order to defuse conflict.

In one of two articles in Section II, "Simulation of the Problem," Martin Shubik states that the value of simulation lies mainly in the bringing together of people and the mutual understanding which results. In the other article, "Take a Longer Look," Milton U. Clauser examines the possible roles of the university—creating a serene atmosphere or tackling problems head on.

Section III, "International Conflict Analysis and Simulation," brings together a number of interesting approaches. I would recommend highly to students of mathematical escalation John Voevodsky's "Modeling the Dynamics of Warfare." The Guetzkow piece about simulations and international relations may be familiar to some readers but will make important reading to others. It demonstrates that simulation can contribute to conflict reduction by fostering increased knowledge and understanding of other actors and thus decreasing the likelihood of misinterpreting their actions. In addition, simulation can pinpoint (and thereby aid in avoiding) any areas of conflict which might arise from a new policy. In another article, Ivo Feierabend hypothesizes that systemic frustration breeds aggression, which in turn breeds internal violence, which creates a predilection for international hostility. Conflict may be averted by intervening in this chain—i.e., by manipulating the levels of systemic violence.

Section IV, "Technology," deals wih the potential feats of computers, including their use in decision making through benefit analysis and related techniques. The Carl Hammer article on interactive electronic systems smacks a bit much of "Star-Trek" reruns, but I. J. Good's extensive piece on computers' future social repercussions is extremely intriguing. The notion that computer-assisted organizations will by necessity move toward greater complexity (his fourth law of thermo-dynamics) bears attention. The nostalgia for von Neumann's lucid and public reporting of reasons for design change is unnerving to those of us who still have not demystified von Neumann.

In general, this volume is useful for understanding the technical innovations in conflict resolution, with emphasis on simulation, events analysis, and information theory and technique. I feel it has ample value for the study of conflict dynamics and the growth of conflict because of the variety of concerns it touches upon. There are studies on cross-national conflict, such as the Feierabend piece, as well as domestic (student) conflict, such as a statistical survey by Raymond Tanter. I would add as another note in its favor that the book does not deal analytically with the traditional inter-nation conflict issues—military structure and weapons management.

Its main failing is its inability to get to the real problem of conflict resolution, possibly the major ongoing problem which confronts conflict research in Europe and North America: the notion that conflict control equals conflict resolution. This is simply wishful thinking. The bulk of the research in the volume deals with conflict escalation and the mechanisms for increasing conflictive behavior. In short, all the authors are operating on a pre-resolution level, thus making the title a bit misleading.

Equating conflict resolution with the reduction or containment of violence ignores the fact that conflict de-escalation could take place in one dimension—such as reduction of violence and the absence of overt hostile or armed activity—while at the same time, other forms of escalation, such as economic sanctions, could increase.

The writings here also reflect a number of other conceptual problems. All the authors seem to assume, at least implicitly, that symmetry exists in conflictive situations. Partners in a conflict—e.g., students vs. an administration, small nations vs. large nations—may differ on so many dimensions that a thorough knowledge of conflict behavior demands better conceptual tools which can account for these differences. For example, the authors could have differentiated between the parties of a conflict on the basis of structure and organization, value systems, and interactive capacity.

Furthermore, the authors here (like many other social scientists) tend to assume that the only "real" conflict that exists, or at least that we can study, is overt and observable. What about the extremely important sphere of latent conflict? A thorough knowledge of the conditions which can reduce inequalities between national or international actors may help avoid future hostile interactions altogether.

I feel that future thought and research, perhaps by these very authors, will show that conflict resolution requires a decrease to "tolerable" limits of inequalities among the conflicting parties. This might require building those structures which facilitate the removal of the asymmetries in value systems, conflict behavior norms, and interactional capabilities.

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A Pre-View of Policy Sciences. By Harold D. Lasswell. (New York: American Elsevier Publishing Co., Inc., 1971. Pp. 173. $7.50.)

In discussing the professional identity of contemporary policy scientists, Professor Lasswell observes:

> In achieving the new identity it has been necessary to overcome the image of a second-class man of knowledge and a second-class man of action, and to perceive that the integrative role of the policy scientist is indispensable to the security and advancement of a world civilization of science-based technology (p. 120).

Throughout his career, Harold D. Lasswell has consistently and persistently called us to the larger challenge. In this book he sets comprehensive tasks, refusing to acknowledge our limitations as roadblocks to their accomplishment. If, perchance, we were suddenly equipped to meet Lasswell's challenges I suspect we would either be on that glory road to utopia or the society would come to a dead stop—a victim of decisional sclerosis brought about by too much planning.

The probability of a quantum increase in rational planning and policy development is, I suspect, relatively low. While I would never wish to belittle Harold Lasswell's impact, most of us maintain narrow interests, quite resistant to integrative pressures. Lasswell calls for the kind of comprehensiveness that characterizes *his* mind and style, but there really is only one Harold D. Lasswell. He is a brilliant man, as is evidenced by this latest book, *A Pre-View of Policy Sciences.* He classifies concepts as though his mind were equipped with multi-powered magnets—pulling units into various categories. He forces himself to be ever more encompassing, bounding ahead but reaching laterally as well. He experiments with integration by intentionally selecting illustrations that are out of the ordinary, that would seldom occur to most of us. And he constantly projects into the future—seeking a return for analysis of the present.

But that is Harold Lasswell. And who are these policy scientists he talks about? Presumably they will, or should, be even more comprehensive and integrative.

> The emerging policy scientist in our civilization is not only a professional in the sense that he combines skill with enlightened concern for the ag-

> gregate processes and consequences of decision. He belongs among the systematic contextualists who are also empirical. . . . the policy scientist is concerned with mastering the skills appropriate to enlightened decision in the context of public and civic order. . . . [H]e is searching for an optimum synthesis of the diverse skills that contribute to a dependable theory and practice of problem solving in the public interest (pp. 12–13).

One might immediately question whether an emerging class of policy scientists is compatible with democratic public order. That is of less immediate concern to me, however, than who these people will be (or are) and where they will be (or are) located. At the risk of insulting Dr. Kissinger, I am compelled to say that I don't know any such persons presently in decision-making capacities. Lasswell does observe that "not many full-time policy scientists can be identified at present," but I have trouble sensing where to look for them in the future. My own set of concepts permits me to envisage a "policy science," but not acting policy scientists (at least as described by Lasswell). I can understand the policy science role of academic advisers who are broadly trained, as well as of decision makers who are told "be comprehensive." What is never made concrete to me in this book is where I should expect to find these super-integrators in our system of political decision making.

The failure to indicate who these policy scientists will be and what they will do detracts somewhat from this volume, but the many other purposes it serves recommends it to political scientists. As with *Public Policymaking Reexamined* (Yehezkel Dror), this book sets the agenda for comprehensive, integrated decision making. One surely gets a feel for the task at hand as Lasswell outlines how policy scientists should look at the world, how they should pursue their tasks, what they have to offer both the "ordinary policy process" and the "constitutive policy process," how they might gain professional identity, and how they might be trained. Those acquainted with Lasswell's work will find much that is familiar here—though often set in a slightly different context or relied on for different purposes. As with Dror, Lasswell develops a strategy of rational problem solving: goal clarification; trend description; analysis of conditions; projection of developments; and invention, evaluation, and selection of alternatives. And the familiar, though slightly modified, "seven power outcomes" (intelligence, promotion, prescription, invocation, application, termination, appraisal) are invoked throughout. He presents a less-tortured and more obviously pluralistic means for identifying goals than Dror does—advising the policy scientist to "search for postulates of sufficient generality to provide guidance in coping with the problems that arise in connection with the shaping and sharing of values" (p. 41). Among other services performed, books of this sort illustrate the complexities of achieving an appied social science—of meeting the challenges outlined by Professor David Easton in his 1969 presidential address before the American Political Science Association.

Lasswell's discussions of professional training are also valuable. Those of us interested in training political scientists to engage in socially relevant policy analysis need to review existing graduate programs. We need to find parsimonious means by which students can learn more about substantive issues and gain experience in decision-making processes. The internship has proved of only limited value in this connection. Lasswell proposes a "continuing decision seminar" that has much to recommend it, particularly, I believe, in graduate training. Each such group would be composed of a "small nucleus of self-constituted members" with a "determination to continue."

Like all the other books of Harold Lasswell, this one will find several audiences. The audience he intends is typically in the future—where Lasswell himself has always been most comfortable. Given Lasswell's integrative skills, a present- and past-oriented audience will also find their values and problems analyzed somewhere within these pages. Those who insist on getting the most for their money, however, should be encouraged to test the Lasswellian recipes. Though some are too rich for the common palate, they can be halved or quartered and still satisfy the creative urge of most of us.

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The German Historians and England. A Study in Nineteenth-Century Views. By Charles E. McClelland. (Cambridge: Cambridge University Press, 1971. Pp. x, 302. $16.50.)

There are many ways of learning about the past; one of the best is to observe it through the eyes of the past. The experience is salutary. How subjective, how dominated by the obsessions and the wishful thinking of their generation are even the most scholarly writers; and how unscholarly are some of the most gifted and the most learned. Consider the authorized and the revised versions of the origins of the Cold War. Consider the perennial debate, never more relevant than in 1972, on the character of American populism: Is it enlightened, or is it obscurantist? The facts, one would have thought, do not change, though new ones may well be discovered. But interpretations, especially influential ones, may in turn become part of the data. Truthful or not, they are truth-creating.

There is no better instance of this process than the varying German interpretations of the English experience between the 1770s and 1914: the development of the professional historian's craft coincided with the period when British government and society were widely regarded, for better or worse, as the world's most advanced. Even by the time of the French Revolution the main lines of German scholarly Anglophilia were visible. They radiated principally from Göttingen, the cradle of German historiography which was the port of entry for British political ideas, and, as professor McClelland might have emphasized more, economic theory and philosophy, too.

There were three principal themes. The first was admiration for British liberties, based partly on swallowing whole the simplicities of Montesquieu and Blackstone, but also on the undeniable freedoms enshrined in *habeas corpus*, trial by jury, a press uncensored at least by German standards, and the self-confidence of propertied classes who had tamed the executive. The second was admiration for British stability and respect for tradition; thus the same aristocratic independence and common law conventions that fired liberal hopes also reassured conservative publicists like Justus Möser. Both schools saw the key to British institutions, and their relevance to Germany, in a shared Anglo-Saxon-Protestant love of liberty.

All these perceptions were reinforced by the wars against Napoleon. Britain was now the defender of Europe's liberties, a role that was read backward, with all the unhistorical enthusiasm of the historicist, to the wars against Maria Theresa, Louis XIV, and Philip II. Professor McClelland writes, "Out of the Anglo-French struggle of these years grew the philosophical, political and ideological connection between Anglophilia and historicism that was to last for many decades into the nineteenth century" (p. 32).

The academic historians of the Restoration period were the principal driving force in German constitutionalism and therefore the chief publicisers of the British example. The majority of them saw in the balanced constitution of eighteenth-century theory the kind of polity they would have liked to see in Germany:

> they desired, above all, to check the arbitrary exercise of power by princely bureaucracies, not replace these bureaucracies with a full-powered parliament. Impeachment, free speech and press, and limited ministerial responsibility were to be the weapons of an awakened bourgeois intelligentsia against arbitrary government, and these were best developed in Britain. What they did not see so clearly—the development of parliamentary predominance and government by interest-oriented parties—was something they did not want in Germany (pp. 78–9).

As before, this selectivity allowed moderate conservatives as well as constitutional liberals to hold England up as an example. It meant that Ranke, who towered above his contemporaries in methodological rigor, could be an Anglophile on grounds of English culture, English society and, above all, foreign policy.

Equally fascinating, but more problematic, is the question to which the author devotes his final section. Why should the consensus of historical opinion have moved, in the course of one generation, from Anglophilia to Anglophobia? In part it was, like all historical revisionism, a reaction against accepted oversimplifications. More important, the emergence of united Germany as a great power implied rivalry with Britain rather than dependence on her balancing role. The *Flottenprofessoren* (navy professors) of Tirpitz's propaganda machine—Hintze and Delbrück, Marcks and Schmoller—were a long way from Ranke's Anglo-German "brotherhood in arms," though they should not be thought of as a hate-group with a single party line (pp. 191–213).

German unification under Prussian auspices also made Britain less relevant as a guide. After 1867 it was obvious (never having been probable even before that year) that German constitutional life would not follow the British example. The existence of the new state made foreign examples not merely superfluous, but offensive. For three quarters of a century England had served as a counter-ideal to the baneful influence of the "hereditary enemy," France. Now, as Germany took her own increasingly illiberal path, parliamentary free-trading England in turn became a counter-ideal. In the hands of a sophisticated scholar, like the Prussophile Liberal Rudolf von Gneist, this emancipation from English apron-strings was healthy (pp. 135–44). In the hands of Treitschke it set a propagandistic tone that influenced a whole academic generation. Indeed, the notorious combination of insecurity and aggressive self-assertion that characterised the political debates of Imperial Germany required a whole gallery of counter-ideals, including the patriotic but liberal Jewish community. McClelland alludes to the alliance of antisemitism and Anglophobia (p. 195), but omits Treitschke's role in this development.

McClelland's net takes in academic historians, constitutional lawyers, intellectual politicians (like E. L. von Gerlach and Lothar Bucher) and freelance publicists of the Right (like Gentz) and the Left (like Marx or Bernstein). He writes with a historian's perspective. The judgments are those of our day, the emphases reflect his subjects' importance in their own time. Treitschke and Ranke therefore rate a chapter each, Engels a few pages. Rather more regrettable is the almost total exclusion of Catholic scholars, even granted that interest in England was commonest among North German Protestants. Acton's friend Döllinger rates a footnote, Adam Müller a few sentences, Görres and Burckhardt are ignored.

Biographical and bibliographical appendices of 55 pages greatly enhance the value of the book, but its chief merit in an age of committed scholars is to submit a particular era of commitment to a critique. As the center of historical studies moved from Hanoverian Göttingen to Prussian Berlin, the scholars' view of the English past reflected their hopes for Germany's future. True, they thus "raised history above the level of dead men and moldering paper" (p. 236), but they also fell into the trap of "backward projections that resulted in distortions of history which ultimately became policy" (p. 212). Intentionally or not, Mr. McClelland has written a very good tract against tracts.

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Hegel: A Collection of Critical Essays. Edited by Alasdair MacIntyre. Garden City, New York: Doubleday and Company, 1972. Pp. viii, 350. $250, paper.

The latest addition to the Doubleday series of critical essays on great philosophers is both a credit to the series as a whole and a valuable addition to the rapidly growing literature on Hegel in English. Alasdair MacIntyre has included a variety of intellectual styles and philosophical approaches to Hegel, making for a highly readable volume with something of interest for anyone interested in Hegel, regardless of his approach or sympathies. The tone of the essays is for the most part detached, but sympathetic to the great German thinker. It is a tone admirably suited to help us, in the editor's words, "recover the Hegel of fact from the Hegels of fiction and . . . recover him as a living philosopher concerned with genuine issues" (p. viii).

The anthology contains four essays not previously published. Prominent among them (and probably the outstanding piece in the collection as a whole) is Charles Taylor's lengthy and insightful discussion of the "epistemological" chapters of Hegel's *Phenomenology of Spirit* (the Introduction and the first three chapters). Taylor combines his clear, analytical turn of thought with a genuine sympathy for Hegel's position, and provides a paradigm of the sort of Hegel scholarship we need now. A second new essay of considerable interest is Klaus Hartmann's "Hegel: A Non-Metaphysical View." Hartmann attempts to interpret Hegel not primarily as a metaphysician speculating about the real as such, but as a philosopher concerned with the formulation of systems of categories in terms of which domains of reality may be "reconstructed" in a manner which is complete, coherent, and satisfying to reason. Hartmann's writing is sometimes difficult, and his philosophical style may make his essay hard going for some Anglo-American readers. But in my judgment the content of the essay justifies the effort. Hartmann's interpretation, however, seems to work more successfully with those works of Hegel which employ a strictly conceptual dialectic (the *Logic*, the *Encyclopedia*, the *Philosophy of Right*) than it does with works aiming at a historical dialectic (the lectures on the history of philosophy and philosophy of history, the *Phenomenology* itself to an extent).

A third new essay in the volume is MacIntyre's own "Hegel on Faces and Skulls," a surprisingly convincing discussion of a section of the *Phenomenology* that is often an embarrassment even to the work's most ardent admirers. MacIntyre argues persuasively that in this section Hegel is defending a conception of the explanation of behavior which constitutes an important challenge to models of explanation based on laws and generalizations. Richard Schacht's "Hegel on Freedom" is probably the weakest of the four new essays, but it too is a helpful discussion of one of the most important and confusing concepts in Hegel's philosophy.

The other seven essays in the anthology have been printed elsewhere. The book opens, after a brief introduction by the editor, with a short, light, and somewhat introductory essay by J. N. Findlay, whose subject is the "mood" or "tone" of Hegel's philosophy as a whole. Findlay portrays the boundless optimism, humanism, and rationalism which form the core of Hegel's outlook. His account makes it clear that Hegel's proper philosophical antagonist is not at all the scientific humanist who has in fact so often rejected Hegel as an obscurantist. Hegel's proper foe is rather a thinker like Kierkegaard (who understood Hegel much better), a thinker who places little faith in the power of human reason to build a world in harmony with man's nature, and who looks upon all attempts to discover order and reason in the world as so many modes of flight from the absurd predicament of the solitary individual.

One of the finest essays in the volume is R. C. Solomon's discussion of Hegel's concept of *Geist* (Spirit or Mind). Solomon approaches this concept via the Kantian conception of the transcendental ego, and he views Hegel's doctrine of a trans-personal "spirit" as the result of a critique of Kant's "first-person oriented" theory of subjectivity. This essay, along with those of Taylor, Hartmann, and MacIntyre, belongs to the philosophical core of the volume.

George Armstrong Kelly's essay on the lordship-bondage theme in Hegel is an attempt to balance Kojeve's "social" interpretation of this motif with an "intra-personal" one. Kelly seems to me to be attacking Kojeve just where his position is strongest. Nevertheless, there is something to be said for the manner in which Kelly reads the lorship-bondage motif back into later stages of consciousness in the *Phenomenology*. Michael Kosok's "The Formalization of Hegel's Dialectical Logic" is perhaps as difficult and obscure as its title would suggest, but is nevertheless just the sort of article likely to disappoint expectations. It seems to me a mistake to suppose that Hegelian logic can be treated in a manner parallel to modern mathematical logic; both are indeed approaches to an understanding of the nature and procedures of rational thinking. But apart from this, it seems to me they have very little in common, and that no attempt to formulate Hegel's logic in a manner parallel to modern logic is likely to prove very illuminating.

Walter Kaufmann is represented in the volume by two essays. One of them, "The Young Hegel and Religion," is a lively discussion of some of Hegel's most accessible and most neglected work, the writings on religion and society which antedate his Jena period. As might be expected, the reader senses at times that it is Kaufmann rather than Hegel who is speaking to us out of the *Jugendschriften*, but it cannot be denied that the young Hegel provides good grist for Kaufmann's mill. The essay is marred, however, by a largely irrelevant and rather unpleasant personal attack on Georg Lukacs.

Kaufmann's other essay is his well-known reply to Popper's hysterical attack on Hegel in the *Open Society*. Kaufmann painstakingly, if polemically, unravels the "quilt" of out-of-context-quotations on which Popper bases his reading of Hegel as a "proto-Nazi," and he shows quite successfully how completely unfounded this reaction to Hegel really is. The essay is Kaufmann at his best. It is from one point of view paradoxical that such an interpretation of Hegel could even be advanced by a mind as good as Popper's, since there is so little to be said for the interpretation itself, and since Popper argues for it in a manner which does not even come close to meeting the intellectual standards any respectable scholar would normally demand of himself. Kaufmann seems fully aware of this paradox, and maintains throughout the essay an attitude of respect for Popper and his philosophical work.

But Popper has certainly not been the last to brand Hegel as a "fascist" and a "totalitarian" and it is doubtful that scholarly replies will soon succeed in stripping such rubbish of its intellectual respectability. The reason for this, I think, is simply that Hegel's social thought contains a profound critique of the sort of liberal individualism on which Anglo-American culture has long prided itself. Those who look upon the liberal individualist ideology as a sort of moral fortress from which all "men of good will" must wage warfare on the "forces of darkness and oppression" have tended to react to the long tradition of Western political thought which criticizes or rejects this ideology by identifying the critics with whatever evil forces happen to be threatening the fortress of liberal individualism at the time. The groundless and anachronistic application of political labels to any critic of liberal individualism is often no more than a defense mechanism used by those who intend to close their minds to the defects of the "open" society. And since such attacks on Hegel and others are not rationally motivated, they are not likely to be silenced by rational replies. But this is not, after all, a matter for too much concern. For the harm done by this liberal manichaeism to the memory of Hegel (or Plato, or Rousseau, or Marx) is of no importance at all compared with the harm it has done and continues to do to a "free world" under its dominion.

Hegel's political thought is treated from another point of view in Shlomo Avineri's concluding essay, "Hegel Revisited." The essay is a little masterpiece, well written and surprisingly light for the amount of information and thought it contains. More essays like it, in fact, might transform "intellectual history" from a term of opprobrium into one of praise. Avineri surveys the history of Hegel's image in Germany from Hegel's own time to the twentieth century, and attempts to evaluate the political meaning of Hegel's thought in its own and later contexts. The Hegel who emerges from Avineri's study is a cautious progressive, a supporter of a kind of enlightened constitutional monarchy which had already ceased to exist by the time both Hegel's defenders and his opponents began treating his philosophy as an apology for the Prussian status quo. Avineri concludes, in fact, that the meaning of Hegel's political thought for its own time can only be understood if we realize that Hegel was "utterly wrong in his understanding of his own *Zeitgeist* . . . Hegel's political ideas were always 'out' in Germany. Hegel never really had a chance" (pp. 347f).

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On Colonialism: Articles from the New York Tribune (sic) and other Writings. By Karl Marx and Frederick Engels. (New York: International Publishers, 1972. Pp. 382. $7.50.)

This edition of Marx and Engels on colonialism is a reprint of the Moscow, Foreign Languages Press edition which came out in the 1960s (undated), with the addition of five items, occupying eight pages of text. The Moscow edition was entitled simply *On Colonialism*, thus avoiding the sloppy reference to the *New York Daily Tribune* which appears in the title of the American edition.

As stated in the Publisher's Note, the selection of texts "was made with a view to presenting the Marxist analysis, as events themselves unfolded, of the predatory colonial policy of the capitalist states and of the national liberation struggle of the oppressed peoples" (p. 7). Hence what is equally important in Marx, the notion of the positive function of colonialism, is neglected apart from the inclusion of the well-known articles "The British Rule in India," "The Future Results of the British Rule in India," and some material from the correspondence.

While condemning the hypocrisy and greed associated with colonialism, Marx believed that it was a necessary experience for the non-Western world. According to Marx's analysis, the non-Western world was characterised by a non-dynamic socioeconomic system capable of perpetuating itself indefinitely in its backwardness. Marx saw the irresistible force of expansionist western capital as the historical means of bringing an end to the stasis of the east and introducing the dialectic of social change. One will not find any editorial reference to this aspect of Marx's views on colonialism in the volume under review.

In general, the editing of the texts is careless, and there has been no attempt with this American edition to take advantage of the vast amount of research which has been carried out in recent years on the topic of Marx's analysis of the non-Western world. Footnote 38 still describes *Jagirdars* as "representatives of the Moslem feudal gentry in the Great Mogul Empire . . ." (p. 355), despite the publication in Moscow in 1958–59 of Marx's notes on Kovalevsky, in which he specifically rejects the description of the Jagirdars as a feudal class. (See, for example, *Sovetskoe Vostokovedenie*, 1958, No. 4, p. 18.)

The footnote to Marx's comments on the religion of Hindustan preserves in all its glory the obscurantism (prudery?) of the Moscow edition. Marx's comments read: "That religion is at once a religion of sensualist exuberance, and a religion of self-torturing asceticism; a religion of the Lingam and of the Juggernaut; . . ." (pp. 35–36). The footnote describes the religion of the Lingam as: "the cult of the deity Siva; particularly widespread among the southern India sect of Lingayat (from the word "linga"—the emblem of Siva), a Hindu sect which does not recognize distinctions of caste and denies fasts, sacrifices, and pilgrimage" (footnote 18, p. 352). This is Soviet obfuscation at its best; what Marx meant by the "religion of the Lingam" was simply the erotic or phallic element in Hinduism, and nothing more nor less.

In one of the texts included in this edition the translation is so bad as to be seriously misleading. For example the sentence "Da kamen die Engländer und erzwangen sich den freien Handel nach fünf Häfen" is translated as "Then came the English and won free trade for themselves by dint of force in five ports" (p. 17). This seems to imply that the free trade was general, rather than restricted to the five ports.

On the whole, however, this volume provides a reasonably handy compendium of the Marxist critique of colonialism, provided complete textual accuracy is not required, and the largely illiterate footnotes are ignored. If one is still looking for ammunition with which to attack Western colonialism this is the place to find it.

The selection includes a number of pieces on the subject of the colonial relationship of England to Ireland, which are of contemporary interest. However, a full-length edition of Marx and Engels *On Ireland* has recently been published (London: Lawrence and Wishart, 1971) if one wants to find out in detail the views of

Marx and Engels on the Irish "national liberation struggle." Otherwise, a far more comprehensive collection of texts from Marx and Engels on colonialism is to be found in the volume edited by Shlomo Avineri, *Karl Marx on Colonialism and Modernization* (New York: Doubleday, 1968).

MARIAN SAWER
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The Justification of the Law. By Clarence Morris. (Philadelphia: University of Pennsylvania Press, 1972. Pp. 214. $12.50, cloth; $3.45, paper.)

This book is a collection of essays organized around the central theme stated in the title. Most of the essays are taken from Professor Morris's previously published work, although they have, in most instances, been revised and, in some cases, expanded. Of the essays which appear for the first time in this book, the principal one is Chapter 4, "Law and Logic." That chapter, like much of the book, is directed toward nonlawyers who are interested in law and its role as a social institution. Morris explains why judicial decision making is not primarily an exercise in deductive logic nor even a form of inductive reasoning. Nevertheless, Morris takes pains to show, through cogent examples, that lawyers are not deluding themselves when they assert that the common-law doctrine of precedent does sufficiently restrict the scope of judicial decision so that it is not what Max Weber called "khadi justice."

The principal point of emphasis of the entire book, however, is that law and the judicial process are not self-contained activities. In Morris's own words (p. 1), "the more that law implements the public's genuine and important aspirations, the more just the legal system becomes." Accordingly, Morris strongly affirms the imaginative use of precedent to realize the public's "genuine and important aspirations" that is exemplified in the judicial opinions of Benjamin Cardozo. He recognizes that there are clear limits to what courts can or even should attempt to do. What he fears, however, is not that judges will abuse their authority to change the law but, rather, that the innate conservatism embodied in the techniques of judicial decision making will unduly inhibit judges from exercising the creative powers they inevitably must possess in a legal system like ours. Morris recognizes, nevertheless, that the complex problems with which our society is confronted—such as those involving social welfare and the accelerating exploitation of our natural environment—require solutions that can only be generated on the basis of substantial empirical research. If, therefore, the public's "genuine and important aspirations" are to be achieved, we must inevitably resort to legislative solutions. Only in that way will it be possible to utilize fully the data that one hopes will be generated by the behavioral and natural sciences. Morris's recognition that the demands for justice in a dynamic society require continual adjustments in legal relationships also leads him to consider (particularly in Chapter 3) the question of the ability of a parliamentary democracy to respond adequately to changing social conditions. He contrasts the views of John Stuart Mill and Herbert Marcuse. Morris himself is a Millian, but he does not dismiss out of hand Marcuse's skepticism about the adaptive qualities of the traditional legal structure.

Professor Morris has had a distinguished career as a legal scholar and teacher. The essays reflect the wisdom that such a man acquires over the course of such a career. As a lawyer, I was particularly interested in Morris's discussion of Chinese law, a subject to which he has turned his attention in recent years. I found particularly stimulating and instructive the contrast he draws between the uses of law and the nature of the judicial process in China—particularly with regard to Chinese criminal law before the overthrow of the Empire in 1911–12—and those that have prevailed in the West.

GEORGE C. CHRISTIE
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Magna Carta: The Heritage of Liberty. By Anne Pallister. (New York: Oxford University Press, 1971. Pp. 133. $6.50.)

Anne Pallister's book is an attempt "to explain the growth, development, and transfiguration of Magna Carta" (p. 6) since the seventeenth century. The result is a first-class study—sensible, penetrating, scholarly, well-written, and exciting. It is also short. A less able writer would have made it longer.

In excellent chapters on the seventeenth century, Anne Pallister shows how the supporters of the cause of Parliament used Magna Carta to strengthen their attempts to limit the royal prerogative. The Long Parliament moved away from Sir Edward Coke's belief in the supremacy of law toward the new doctrine of the supremacy of Parliament. Meanwhile the Levellers and Independents united to oppose the radical claims of Parliament. The Levellers "developed a constructive program of reform based upon Magna Carta, which they elevated to the status of a political bible" (p. 15).

The latter part of the century saw the resumption of the conflict between the prerogative powers of the crown and the insistence of the Whigs upon the sovereignty of the law and the customary rights of the people. The Declaration of Rights of 1689, another symbol of liberty, was called the "Second Magna Carta" because it seemed to proclaim the triumph of the principles of Magna Carta. The Revolution of 1688 made it certain that supreme power was to lie in the hands of Parliament.

The early eighteenth century saw the extension of the powers of Parliament and the steady decline of the importance of fundamental law. At the same time, appeals were made both to Sir Edward Coke and John Locke when acts of Parliament seemed to threaten individual rights. Whig and Tory disputes multiplied about the old issues of liberty, the origin of Parliament, the nature of the constitution, and the corruption of politics in the age of Walpole and Newcastle. Late in the century the authority of Parliament was challenged both by the radicals in England and by the American colonists. Camden and Chatham believed that the actions of Parliament must be limited by fundamental law. Blackstone argued that power resided in Parliament. "What the Parliament doth, no authority on earth can undo." He did admit that there were rights, such as personal liberty and property rights, that he traced from Magna Carta, above the power of Parliament.

Anne Pallister's comments about the reformers of the eighteenth and early nineteenth centuries (John Wilkes, John Cartwright, William Cobbett, and Sir Francis Burdett) are interesting and excellent. She remarks that

> during the controversy surrounding Wilkes, Magna Carta was widely used both to justify ancient rights and as a slogan calculated to stir the patriotic emotions of the populace in Wilkes' favour. By this time the Charter had become totally divorced from its historical setting and it is doubtful whether half of those who referred to it had any personal knowledge of its contents; they merely read back into it whatever political maxim or precedent they required (p. 62).

This has often been true.

In "The Charter Attacked" (Chapter 6) the author shows how many men in the eighteenth century abandoned the appeal to history in favor of an abstract natural law that stressed the rights that belong to all men. Thomas Paine, for instance, departed from the common-law tradition and insisted that every generation has a right to act as it sees fit. Edmund Burke stressed the feudal idea of the Charter and rejected Coke's idea that it was a statement of natural liberties. There are excellent passages (pp. 82–88) explaining how Jeremy Bentham and John Austin developed a justification for parliamentary sovereignty by substituting a principle of utility for earlier philosophical theories based upon historical or natural rights theories.

The author entitles the last chapter of her book "The Charter Repealed." By 1970 only 4 chapters of the 37 chapters of the Confirmation of the Charters of 1297 survived. (The Confirmation is printed in an Appendix.) The other 33 were repealed between 1828 and 1969 as a part of an attempt to reduce and clarify the laws of England. Thus Magna Carta has been "repealed." It is also true that acts of Parliament cannot destroy Magna Carta as a symbol of freedom, a milestone on the road to liberty.

GOLDWIN SMITH

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Friedrich Meinecke and German Politics in the Twentieth Century. By Robert A. Pois. (Berkeley: University of California Press, 1972. Pp. x, 164. $7.50.)

This earnest, somewhat labored volume attempts to relate the "political thinking" of a famous German historian to "German internal politics during the twentieth century."

Friedrich Meinecke, who died in 1954 at age 92, was a major figure of German historiography and the modern school of "historicism." Student of Dilthey, Sybel, Droysen, and Treitschke, contemporary of Max Weber, Troeltsch, and Rickert, Meinecke had a career that spanned six decades, and he remained active to the end. One of his best-known books was a slender 1946 volume in which he reflected on "The German Catastrophe." His most important scholarly works were "Machiavellism: The Doctrine of *Raison d'État* and Its Place in Modern History" (1924) and "The Origins of Historicism" (1936). He has already been the subject of several books written in Germany and the United States.

While Meinecke is known as an important historian of ideas and theoretician of "historicism" (which he defines as "replacing a general and abstract contemplation of human affairs by an individual one"), he is also often held to have been one of the moral leaders of the attempt to understand and overcome the origins of Nazism in Germany, and a man especially open to revision of his political thinking. In this interpretation, for example, Meinecke is held to have moved from being a proponent of Prussian-German authoritarianism to the position of tolerant democrat, even as early as the years of the Weimar Republic.

It is such interpretations which Professor Pois, himself an historian, is concerned to examine—and reject. He finds Meinecke's political thinking to have been muddled, especially in misunderstanding the characteristics of Nazism and failing to provide a "meaningful approach for the investigation of Germany's past" (p. 143). He writes of Meinecke's movement from "statism" to a "confused melange" of ideas, ultimately oriented around a vague and apolitical "cosmopolitan Romanticism" which was neither intellectually convincing nor based on an awareness of the political realities of modern mass movements. (p. 48). According to Pois, not only was Meinecke frequently in "a curious sort of nonideological proximity . . . to the extreme Right" in coping with the collapse of the Empire, the radical attacks on Weimar, and the emergence of Nazism—he was also a "curiously nationalistic Goethean cosmopolite adrift in a world of mass chiliasm and mass politics" (p. 118).

Despite the author's frequently unclear language, and his intolerably loose use of concepts such as "class" (he often refers to Meinecke as a representative of an undefined "class," and on at least two occasions seems to dismiss the whole rationale for his own study by asserting that Meinecke, after all, "was neither an ideologist nor was he capable to understanding [sic] an ideology"; he "was merely a rather conservative, very cultured, rather pleasant, and somewhat prejudiced German burgher" [p. 114]), the book does draw attention to some important themes. Pois shows, for example, that Meinecke's persistent view of the Hitler movement as "divisive" blinded him to the integrative function of the Nazi appeal for "unity" and a new "*Volksgemeinschaft.*" Presumably, had Meinecke himself been more aware of the dilemmas of reconciling diversities within a body politic, and less a "bourgeois German" who fundamentally longed for the "unity" of Imperial times, he would have been able to understand the powerful appeal of a movement which promised a unity that was classless. Even Meinecke's 1946 reappraisal of German history in light of the Nazi phenomenon is shown by Pois to have been quite unclear and actually beside the point, for it reflected more the confusions of a "decent German" than a persuasive interpretation of the roots of the German problem.

Professor Pois' work, based on a doctoral dissertation, hardly settles the question of Meinecke's political development, despite its contributions. Much less does it shed great light on what is surely a fundamental theme: the relationship between German humanistic scholarship as a whole since the "discovery of history," and political conceptions. The relationship between Meinecke's "historicism" and the confusions in his political thought is not clarified by the author. Insofar as he refers to this theme, Pois seems to think that Meinecke should have been a more consistent historicist, and that this would have helped him understand the temporality of phenomena (and, therefore, the appearance of new phenomena such as "mass politics"). Such assumptions are popular, but one wonders whether they are anything more than an intellectual fad. Humanistic scholarship as a whole may be endangered, not because it is insufficiently attuned to change, but because its own endeavors may be debunked as the mere luxury of a particular time, place, or class. While one is not convinced by Professor Pois' book that a study of Meinecke would be the most appropriate way to begin a clarification of the assumptions of "historicism," the author might have presented more clearly the relevance of that way of thinking to Meinecke's political views.

George K. Romoser
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The Defense of Gracchus Babeuf Before the High Court of Vendôme. Edited and translated by John Anthony Scott. With an essay by Herbert Marcuse. (New York: Schocken Books, 1972. Pp. 120. $1.95, paper.)

Gracchus Babeuf was not a major political philosopher. What then is the value of this volume for those who have no special interest in French socialist thought? Besides providing the reader with a neat synthesis of primitive communistic philosophy based on total equality, this work contains an elaborate argument in defense of conspiracy against the state. Specifically, Babeuf attempts to convince a court—appointed by the government which he has sought to overthrow—that he should not be punished for plotting against the established regime. Herein lies the interest of the *Defense* and its "relevance" to an important contemporary issue.

Babeuf's defense is based on (1) a redefinition of the concept of "conspiracy," and (2) an argument for innocence by association.

(1) The charge of participating in a conspiracy is unfounded, because, according to Babeuf, "a conspiracy not directed against the people cannot be a conspiracy at all" (p. 28). A conception of conspiracy which equated it with the intent or act to overthrow any "established" government, would condemn people to remain subjugated to unjust regimes, and is, therefore, unacceptable. Likewise, "to work for

the overthrow of a constitution freely adopted by the people" is not necessarily conspiratorial, for "the people might, with apparent freedom, have adopted a radically vicious Constitution" (p. 35). Rather, the only truly subversive act would be a conspiracy "to bring about the overthrow of legitimate authority"—i.e., authority established according to "the true principles of popular sovereignty; which governs in accordance with those principles; and which dedicates itself to the welfare of the nation, to the enhancement of its glory, to the defense of its freedom" (p. 36). Hence, Babeuf argues, his intent to overthrow the French government established by the Constitution of 1795—a freely adopted but "vicious" constitution—is not a conspiracy. Rather, he and his associates were motivated by the desire to replace an "illegitimate" authority with a regime calculated to achieve both "the common welfare, which is the purpose of social existence," and "the happiness of the greatest number, which is the purpose of the Revolution" (pp. 44–45).

(2) Most of Babeuf's defense is devoted to a summary of his ideas and a demonstration of the identity between these conceptions and those advanced by some of the most "revered" and "admired" French political philosophers. This tactic rests on Babeuf's belief that "the man who wills the end also wills the means to gain that end":

> It is clear that these changes [proposed by the "conspiracy of equals"] cannot be achieved without the overthrow of the existing government and without measures to eliminate whatever obstacles stand in the way. This overthrow of the established order, with whatever repressive measures accompany it, is a necessary consequence of willing the original goal (p. 48).

Thus, if we are to accept Babeuf's argument, he and his comrades are on trial for their ideas—ideas which were first propounded by such "eminent men" as Rousseau and Mably "who command the respect of everyone" (p. 63).

The reasoning within Babeuf's *Defense* is not rigorous. For instance, legitimate authority is, effectively, made equivalent to the form of government he prefers—i.e., a communistic system. Every other system is illegitimate, because even if supported by the overwhelming majority of citizens, Babeuf could nevertheless claim the people were misinformed or had been tricked. To cite another example of the weakness of Babeuf's reasoning, the idea of innocence by association is not only as logically untenable as the more common guilt by association arguments, but also is somewhat inapplicable. For after all, Rousseau, as well as Babeuf's other philosophical ancestors, did not attempt to develop a revolutionary organization and prepare to overthrow by force the established regime; yet, this is precisely what the "conspirators" did seek to do, and the communality between their ideas and those of their predecessors is irrelevant.

But to regard Babeuf primarily as a thinker and a philosopher seems somewhat erroneous. His rather primitive communism and concept of pure egalitarianism is largely derived from the works of others, as he himself repeatedly claims in his *Defense*. Babeuf's major contribution was as a political organizer. He sought to develop a party, an elite which would spread the "proper" ideas among the "masses" and convince them of the need to seize power for themselves—i.e., what in modern terminology would be referred to as the "vanguard of the proletariat." It is regrettable that neither the editor, John Scott, nor Herbert Marcuse devoted any of their commentary to this important aspect of Babeuf's historical role. In addition, their otherwise insightful and useful remarks about his life and philosophy are somewhat marred by painting his picture in exclusively glowing terms. We are told how Babeuf rises from poverty to play the noble role of leading a revolutionary struggle against an unjust and evil government. We see an uneducated man who learns to be a philosopher; a man motivated entirely by his desire to do good and to make a better world for his compatriots. But even so important an event as Babeuf's attempted suicide following the Court's verdict—which might be regarded as a rather unheroic act for someone presented to us as having heroic stature—is not mentioned either by the editor or by Mr. Marcuse. Rather, Marcuse cryptically refers to this event as "the horrible scenes following the pronouncement of the death sentence" (p. 103). In summary, this book is to be valued not for its rich theoretical insights, but rather for its presentation of the interesting process by which a condemned man attempts to convince a court (appointed by the government he sought to overthrow) that he should not be considered guilty as charged.

WILLIAM R. SCHONFELD
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Xenophon's Socrates. By Leo Strauss. (Ithaca: Cornell University Press, 1972. Pp. 181. $8.50.)

In this book Professor Strauss offers an account of Xenophon's *Memorabilia*, *Apology of*

Socrates, and *Symposium*. The account is textual analysis, narrowly construed. It is devoid of footnotes, and ignores the mass of secondary literature on Xenophon and the Socratic Problem generally: no attempt is made to assess the accuracy of Xenophon's portrait of Socrates, nor to relate it to the differing and often conflicting accounts in Aristophanes' *Clouds*, Aeschines of Sphettos, and most especially, Plato. Professor Strauss also ignores such questions as these: Is the *Apology* attributed to Xenophon spurious? Was the *Memorabilia* directed, as Cobet thought, not to the speeches of the original accusers, but to the "Accusation of Socrates" written by the sophist Polycrates well after the trial? Was Xenophon's portrait of Socrates largely derivative from Plato's, as Burnet thought? Failure to take account of the scholarly tradition dealing with Xenophon is a serious flaw in this book.

Nor is the flaw redeemed by Professor Strauss's analysis, which is generally hard to follow, and often insensitive. A sample will do. At *Memorabilia* IV, iv, 19, the sophist Hippias is made to agree with Socrates that there are unwritten laws made by the gods for men, for example, honoring gods and parents; but Hippias denies that the prohibition against incest is among those laws, on the ground that some nations transgress it. Professor Strauss infers that, "He obviously implies that the unwritten laws are never transgressed and in particular that the laws enjoining honoring the gods and the parents are never transgressed by anyone; whether he thinks so from innocence or from the lack of it, we are in no position to tell" (p. 113). The obviousness is unobvious. Hippias is proceeding on an assumption common to his time, that divine law is, not untransgressible by individuals, but universal among peoples—an anticipation of the later association of *jus gentium* with *jus naturale*.

R. E. Allen

University of Toronto

Development Assistance in the Seventies: Alternatives for the United States. By Robert E. Asher. (Washington, D.C.: The Brookings Institution, 1970. Pp. 248. $6.95.)

This study considers the future of American aid to low-income countries. It covers a broad range of topics, including the rationale for aid, important aspects of the development process, the case for a multilateral program, and foreign trade and investment. The author, a Senior Fellow in the Foreign Policy Studies Program of the Brookings Institution, concludes with recommendations concerning the nature, scope and administration of American aid.

As a background to the discussion of aid, Asher outlines the changes occurring in the socioeconomic, political, and military environment in America and internationally. Unfortunately, he does not consider the fundamental changes in communication, technology, and ideas; he concentrates on the more visible disturbances, such as poverty in the United States and protests, and on policies for dealing with these symptoms.

Asher assumes "that the economic, social and civic development of low-income countries . . . is in the long-term interest of the United States" (p. 2). Whether this is so is a cardinal question. It has not been answered convincingly, and this has affected America's willingness to give, as well as practically every other aspect of her aid policy, including the relations with the recipient countries. Often, the relevant arguments contain a number of steps. Thus, economic development might or might not increase a country's political and social stability; either outcome might or might not enhance the country's disposition for international accommodation; and this, in turn, might or might not serve the interests of the United States. Sometimes, one or more steps are missed; the effects, economic and political, of using funds for aid or, alternatively, for other purposes are not compared; or the development abroad is confused with the aid motives or interests of the donor. Some might argue that too big a jump is involved even in the argument that "recognizes the drive to conquer poverty abroad as a logical extension of the same pressures and considerations, moral and material, that make urgent the conquest of poverty . . . at home" (p. 103).

Like the corresponding chapter of the Pearson Report, Asher's chapter on the rationale for aid raises at least as many new questions as those it tries to answer. The reader who is concerned about the future of aid will find little help for persuading others that aid serves the long-term interests of America. Asher recommends educating Congress about development assistance and persuading the President that vigorous leadership in favor of aid is in the best interest of the United States. Alleged weaknesses in America's leadership have been blamed too easily for the flagging of aid, while the experts have failed to provide unequivocal arguments in its favor. Attempts to educate Congress might sharpen the question of the relation of aid to United States interests and have a backlash effect.

Asher quotes widely diverging projections of income growth in the poor countries and asserts that a 4 per cent yearly increase in per capita GNP can be obtained in the 1970s. He does not examine the conditions in which such a rate of growth might be feasible and desirable. Various rates of change in economic and social factors affect incentives and responses differently, and they may seriously qualify the possibility of obtaining given rates of growth in income; also, they have a different bearing on welfare, on political and economic stability, and on a host of other developments. Asher is satisfied that in developing countries income inequality will decrease. However, the very creation of a middle class as the rate of development increases, combined with advances in communication, might enhance interpersonal comparison and lower welfare. Asher views population growth mainly as an obstacle to increasing per capita income. But the rate of population growth reflects also social factors and goals; it has important implications regarding the relative shares of world resources which the various nations control, as well as possible claims for redistribution; and it is related to the world problem of growth of agricultural and manufacturing production, pollution and resource depletion, and the constraints on growth and its structure.

In his discussion of private foreign investment Asher might have provided some indispensable background: often, in the poor countries the demand for capital is low, like the supply, and capital earns a higher net return in the wealthier countries. This explains why private capital must be enticed by the poor countries or induced by the advanced countries to migrate. It underlines the importance of policies to raise the demand for capital in the developing countries. It also suggests that "vigorous promotion" of foreign investment by the United States might involve a substantial aid element, and, since such investment might also involve political and other risks, in many cases straightforward aid might be preferable.

Aid from the West, especially the United States, has been floundering. The spirit as well as the flesh has been weak, while the arguments advanced in support of aid have proved insufficient. Asher's stand and efforts to convince are, in many cases, based on personal preference and individual value judgment, rather than rigorous analysis. This kind of approach is becoming more common in the literature on development and aid as it is being increasingly recognized that the problems and goals are more than strictly economic or political. The power of aid evangelism remains to be proved.

S. G. TRIANTIS

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Revolutionary Politics in Massachusetts: The Boston Committee of Correspondence and the Towns, 1772–1774. By Richard D. Brown. (Cambridge, Mass.: Harvard University Press, 1970. Pp. 282. $10.00.)

During most of the eighteenth century, the colony of Massachusetts was a stable society—indeed, a deferential society in which plain citizens followed the leadership of men of superior social and economic status. And yet when the Stamp Act, the Quartering Act, and other assertions of British power in America became manifest in the years from 1765 to 1772, Massachusetts took the lead in fomenting revolution. The beginnings of revolution are traced briefly in two chapters of Professor Brown's study. His primary interest is in the ideology of revolution in the Bay Colony, and particularly in the political techniques the revolutionary forces evolved.

Unlike the birth of most new nations today, the movement in Massachusetts was a revolution of continuity of thought, as it drew on a long-established rationale. This was the radical-whig conception of balanced government that the colonials borrowed from England, a tradition that reached back to Machiavelli and James Harrington among others—whig in the sense of ideology, not party. Much of the revolutionary rhetoric stemmed from the classical whig notions of "virtue" and "corruption." The domineering royal Governor, Thomas Hutchinson, was identified with corruption of the whig balance of executive and legislature; and the colonial cause was portrayed as an effort to restore the balance, and thus virtue in the system. The ultimate result was the American experiment in "republican government." But only later did liberal, Lockean ideas come to the fore.

The crisis also generated new forms of political action, and most of Professor Brown's study is devoted to this aspect of the revolution. The chief device was the "corresponding committee," first launched in Boston; and the Boston committee became the central, coordinating engine of political activity throughout the province, as other committees emerged in town after town. The Boston group not only initiated leads for action, but also provided encouragement, reassurance, and reinforcement. The movement also generated public assemblies, loose conventions composed of members from different towns (one of which included delegates from 100 of the 250 towns in the province), "instructions" for action at official town meetings, and ultimately county conventions. Men like James Otis, Samuel Adams, and Josiah Quincy, Jr. took the initiative in propaganda and rationale—Bostonians all. The famous John Hancock came in later.

Although Professor Brown's study is sometimes prolix and repetitious, it is based on thorough and often sophisticated research as he carefully analyzes the first stirrings of the first colonial revolution.

WILLIAM NISBET CHAMBERS
Washington University

Free Press and Fair Trial. Edited by Chilton R. Bush. (Athens, Ga.: University of Georgia Press, 1970. Pp. 133. $3.75.)

At a time when the subject "free press" brings quickly to mind disputations on the legal and political status of the Pentagon Papers and protection for confidential news sources, it bears noting that another part of the cacophony of opinion on the relationships between government and media has not gone away. This is the free press-fair trial debate which, as one might expect, ebbs and flows in intensity. At the time of this writing the tide seems to be out, but the elements of the debate remain, involving what the late Justice Frankfurter might have called a "clash of rights." That is, the controversy thrusts to the foreground the continuing conflict between the public interests in free expression and due process of law.

The book under review was published near the end of the last round of national debate on the subject, and I have the impression that not very much use has been made of its findings, certainly not among political scientists. Perhaps the book will be remembered for the next round, but for now it may be that the volume has fallen victim to cyclical relevancy. Nonetheless, there is much here of interest to students of the judicial process. The studies which comprise the little volume were written by Fred S. Siebert, Walter Wilson, and George Hough III, each a student of journalism and communications. Their work complements some of the data and conclusions of Professors Kalven and Zeisel in *The American Jury* (Chicago: University of Chicago Press, 1966), even though the two groups of scholars employed somewhat different methodologies. Looking for the principal factors influencing jury verdicts in criminal trials, Kalven and Zeisel evaluated questionnaires completed by 555 judges reporting on 3576 jury trials. In the volume under review, efforts were made employing this and other methods to explore the impact of various kinds of pretrial publicity on jury deliberations and verdicts, a factor not really treated by Kalven and Zeisel. One of the studies in the Bush volume surveyed trial judges' opinions on prejudicial publicity, another noted the frequency and intensity of press coverage of felonies and resulting trials, and a third analyzed the findings of several jury simulations.

Taken together, the three suggest two major findings. First, the commission of most felonies and the great majority of trials attract very little newspaper attention. For instance, of 11,724 felonies committed in New York City in January of 1965, the editors of the New York *Daily News* chose to report only 41. The smaller number itself says nothing of course about the intensity of publicity accorded "newsworthy" crimes and trials; indeed, the impact on the trial process could be significantly greater with such selective coverage. No account is taken, however, of radio and television coverage, and this omission is a weakness, given the extent to which television is the main news source for most Americans. Second, a single pretrial cognitive element—a defendant's criminal record—will very probably have a disproportionate effect on the jury; its members are more likely to return a verdict of guilty when they know the accused has committed offenses in the past. Presumably, news of past criminal acts may result in an almost unconscious conclusion of guilt by the potential juror. It is also significant that knowledge of this record of prior violations can easily survive in the minds of potential jurors from initial exposure to this information to an eventual rendering of the verdict. Other kinds of pretrial publicity, such as publication of a confession or a description of evidence later held inadmissible at the trial, either do not survive or have little measurable effect on the verdict. These findings thus tend to weaken the argument of those who advocate a broadening of informal agreements between the media and government to discourage "trial by newspaper." They will certainly weaken the forces of those who go farther by advocating the application of legal sanctions to reporters and policemen who violate rules on pretrial publicity. With newsmen especially, the state would be required to show a compelling need for the particular infringement on free expression, and this will now be more difficult to establish except of course in the case of a defendant's criminal record and in those rare instances of intimidation of jurors by media.

There is a collateral question, however, which the authors overlook. Assuming that tentative conclusions presented here do indeed prove in further studies to be sound ones—that is, if it is later demonstrated conclusively that very little pretrial publicity is prejudicial against a defendant—this does not mean that current legal rules and the few informal agreements in this area ought to be relaxed. There is, after all, the matter of the internal, as well as external, perspectives on an operable legal system. If there is the appearance of injustice when mounds of pretrial publicity do arise in selected trials, the legal system risks a diminution in the legitimacy accorded the trial process. This risk could be much higher if some of the procedural requirements were slackened. Thus, public policy decisions in this area may be more influenced by symbolic concerns than established fact.

In summary, the volume adds appreciably to the stores of knowledge in this area and deserves attention from students of the trial process, especially jury behavior.

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Big Brother's Indian Programs—With Reservations. By Sar A. Levitan and Barbara Hetrick. (New York: McGraw-Hill, 1971. Pp. 214. $8.95.)

The relationship of the federal government to American Indians has never been easily summarized. Nor can it be today. For limited purposes, a comprehensive description of functional activities on Indian Reservations is of use. This is such a book: anyone intending research on a reservation will find here an accessible description of most active Reservation programs, including brief histories of these activities, their evolution, and recent levels of funding. These program reviews include touches of what the Bureau of Indian Affairs might do "better" in the view of the authors at what the Bureau supposedly intends to do.

In light of the high quality of Professor Levitan's previous published work (*Federal Aid to Depressed Areas* and *The Great Society's Poor Law*), and his well-publicized testimony before many legislative committees of Congress, this volume is a disappointment. There is no internal indication that any but official data sources, (e.g., Indian Health Service obligational authorities, Bureau of Indian Affairs internal memoranda, statutes) were consulted. It is not revealed if the authors visited a reservation or talked with an Indian. They caution officialdom to remember that not all wisdom is to be revealed in Washington. The same caution can be voiced as regards evaluation of Indian programs; not all assessment can come from the Federal City either.

Criteria of assessment and evaluation remain implicit, murky, and official. Thus we are told the industrial park development in the Gila River Indian Community (not the Pima-Maricopa Reservation which they inaccurately call it) is "relatively viable." Relative to what? Similarly, the tourism complex at the Warm Springs Indian Reservation is thought to be "viable." What, one wonders, is the basis of such conclusions. The Warm Springs operation in Oregon is regarded by the Bureau as exemplary. An analysis might well have gone beyond an official view so as to enlighten students of development and mobilization about the political and social impact of virtually eliminating unemployment in a community where previously more than half the adult population sought work unsuccessfully.

No mention is made of the sizable urban "relocation" program of the Indian bureau, which when ended in early 1972, was at the $100-million-dollar annual level. Although that effort to press Indians to make their way in the slums of our great cities was going full throttle when this book was researched, the authors pass it by altogether.

The great bulk of the book, then, is a description of those functions which are relatively stable, politically accepted, and supported in Washington. The resulting analysis is ahistorical and apolitical. More egregiously, it is a summary without criticism.

The complex rage many Indians feel about BIA programs is left out. The BIA's adjustments to Administration pressure is nowhere mentioned. The intramural battles of the Indian bureau are omitted from the analysis, as are local community hostilities toward neighboring Indian settlements. If such matters were beyond their intentions and somehow therefore excusable, the failure to analyze extant subeconomies within reservations is not so. One central element, commercial life pivoting on the trading post, is nowhere depicted. The low multiplier effect within the reservation is one of its crucial economic features. When this book was written these trading post operations had received no official attention. Within months of publication the configuration of pressures had shifted. Thus do reviews of programs based only on official sources suffer, even in the short term.

A more comprehensive treatment of the factors affecting, indeed moulding Indian development is still needed.

JOHN E. CROW

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The Private Nuclear Strategists. By Roy E. Licklider. (Columbus: Ohio State University Press, 1971. Pp. 213. $11.00.)

"The Private Nuclear Strategists"—what a rich and fertile topic for research! For these are the men (and women?) who "have written most of the new extensive literature on strategy and disarmament since World War II; the new language and terminology of deterrence is largely a product of their work . . . they have conducted a public strategic debate over the past ten years that has significantly raised the general level of public information and debate and that has at least accompanied, if it is not the sole cause of, major changes in strategy by the American government" (p. 8).

Roy Licklider divides the strategic community into three groups: the professional military, civilians employed by the government and civilians outside of government. It is this latter group, the private nuclear strategists, who are the subject of his Ph.D. dissertation. Licklider is correct in stating that "our knowledge about the inner workings of the strategic community is rather scanty" (p. 7) and "that we know remarkably little about the individuals who make up this group" (p. 9).

Unfortunately after reading the book we know little more than before. The study is wooden (there is no real feeling for the substantive content of nuclear strategy) and bloodless (it lacks any real sense of who these people are, what they are really like, what their relationships are with each other, or with their employers or how they view their work).

Licklider's population consists of 491 private civilian strategists who have written at least one book or three articles on nuclear weapons policy between 1946 and 1964. The 191 respondents (only 39 per cent) who completed his mailed 11-page questionnaire constitute the sample. In addition, 16 personal interviews were conducted with respondents.

The central hypothesis of Licklider's study was that "the private nuclear strategists were divided into a few distinctive subgroups or communities" (p. 14). He expected "that communication would be largely *within* rather than *between* communities and that the little intercommunity communication that existed would be largely restricted to influentials" (p. 15). The data fail to substantiate the hypothesis of "communities within the population."

The study illustrates one of the most disturbing trends in contemporary political science, i.e., blind or crude empiricism, infatuation with methodology and quantification solely for their own sake. Without any underlying broad organizing formulations or conceptualizations, factor analyses with verimax and quartimax rotations can be just plain data grinding. J. Scott Armstrong has pointed out, in an article aptly subtitled "Tom Swift and His Electric Factor Analysis Machine" (*The American Statistician*, 21 [December 1967], 17–21) that "it is now much easier to perform the factor analyses than to decide what you want to factor analyze."

The author performs sophisticated factor analyses of a "binary choice matrix, using the Cholesky decomposition method, a square root technique and varimax rotation of factors with one or more high loadings" (p. 32). Out tumble two tables with 6 and 20 factors respectively which show us that Thomas C. Schelling is linked with Henry Kissinger on one factor, with Donald Brennan on another, with Walter Millis on yet another and also with Albert Wohlstetter on a fourth factor, and that there is a Paul Ramsey factor. But the possible reasons for or implications of such linkages are never discussed. Are Schelling and Kissinger linked because they both taught at Harvard, or because both have been frequent government advisers? Why is it that Donald G. Brennan and Jeremy J. Stone, whose strategic views are now so disparate, constitute a factor? The tables are simply presented, with no attempt to probe the relationships, or to uncover the underlying factors.

In thinking about the private nuclear strategists and about the issues on which they have been really effective, two items seem to be of overwhelming importance. One is that at one time or another most of these men were associated with the RAND Corporation. Although the RAND Corporation appears six times in the index, it is never discussed in any depth or detail. Nor is any cognizance taken of the association between the civilian strategists and the "think tank." In a similar vein, the Daedalus volume which resulted from the 1960 Summer Study on Arms Control is probably the single most influential volume on arms issues to date. Although many of the subjects of the book were associated with the study and indeed many of their seminal writings appeared in that volume, Licklider makes no mention of either the Summer Study or the resultant publications.

In addition to mindless factor analyses, most of the fifty tables sprinkled throughout the work are admittedly not statistically significant. Even where statistical significance is claimed, a large number of the tables appear meaningless. For example, in Table 31 we are told percentage figures total across but they only total 86%; in the same table, with an N of 9 and category percentages of 33,11,33 and 11 we are told the significance level is .02; Table 34 finds a significance at the .01 level for cross category percentages of 8,8,6 and 8; and Table 38 quotes a "total sample of 145" based on the Ns of 51,14,15 and 45. In short, senseless data manipulation is here compounded by carelessness.

We are left pretty much where we started:

> Real insight into and understanding of the members of the strategic community, their power and their influence in nuclear strategy and disarmament, their ideas, goals, aspirations —all this remains virgin territory, virtually unexplored by *The Private Nuclear Strategists.*

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The Accessible City. By Wilfred Owen. (Washington, D.C.: Brookings Institution, 1972. Pp. 150. $6.50.)

Wilfred Owen, a transportation economist, has brought together in this slim volume a good deal of useful information for the student of urban affairs. He reviews transportation programs and problems, urban development activities, and comprehensive planning ventures in the United States and a number of other nations, both industrialized and developing. The comparative perspective is particularly welcome given the growing interest of urban specialists in comparative analysis. In examining urban transport and development issues in a comparative framework, Owen emphasizes the extremely powerful impact of the automobile on urban development, and thus on the urban political system, in every urban society. Regardless of whether government has encouraged or discouraged automobile ownership, built vast or meager highway networks, and succored or starved public transportation, the automobile has fundamentally altered the urban pattern, promoting decentralization and mobility, and at the same time causing congestion and health hazards.

Owen treats the demand for transportation in metropolitan areas as a function of the pattern of urban development. Thus, he reminds us that demands for public goods and services such as highways, parking facilities, and mass transport systems are not immutable givens, as some models of the political process seem to assume. Demands result from complex and dynamic social, economic, and political interactions. In the case of urban transportation, Owen concludes that governmental strategies designed to alter the demand for transportation services are likely to be more successful than are mere responses to the existing or forecast patterns of demand, especially for automotive transportation. Thus, he argues for more comprehensive planning and stringent public controls on the location of houses and jobs to create new urban environments that are both accessible and livable.

Owen does not examine the political processes that have contributed to the creation of the existing urban pattern of development. He does not attempt to analyze the complex differences in political culture, national history, institutional arrangements, intergovernmental relations, taxing systems, size and scope of the public sector, and other factors which help explain both the urban policy patterns of different nations and their capabilities to undertake the kinds of programs that he advocates. As a result, the political analyst will find more questions than answers in this volume; but both the questions Owen raises and the information he presents provide a useful starting point for more systematic comparative analysis of the politics of urban transportation and metropolitan development.

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Reapportionment in the 1970s. Edited by Nelson W. Polsby. (Berkeley: University of California Press, 1971. Pp. 296. $10.00.)

Those who think reapportionment is a dull subject which has attracted pedestrian minds should take a look at this collection of essays by first-rate scholars, each followed by a thoughtful discussion by a well-qualified commentator. For my part, I approached the book with definite preconceptions. I felt that the Supreme Court had dealt with apportionment problems in a mechanistic way, assuming that a bit of tinkering here and there would produce the desired results. The Court's response to reapportionment had also been simplistic, for nowhere except in minority opinions and stray dicta had it faced the basic complexities of representation. For example, the Court never recognized that American government has traditionally provided multiple points of access to decision making, and that groups losing in one political arena could be doing quite well in another. More specifically, the opinions of the Court showed little understanding of the legislative scene and of two points in particular: (1) In numerous legislatures the major line of division was not territorial but partisan, so that the real question about apportionment was not the one-man/one-vote rule but the effect on the partisan division. (2) Recruitment and career patterns may have more to do with effectiveness in the legislature than do numbers per representative. My own impression is that rural members are likely to be disproportionately effective, because they, more than urban members, are recruited from the capable membership of their communities, have few career alternatives, and tend to stick it out in the legislature.

Given these dim views, I was delighted to find corroboration in the essays by Robert Dixon and Alexander Bickel in this volume. From Dixon, "We expect far more from the election system than mere filling of legislative seats . . . A further goal . . . is that there be room in the system for all significant interests to acquire spokesmen—preserving the right of all to be heard, if not to be in control" (pp. 10–11). And more from Dixon: "Wielding one man, one vote, like a meat-ax, the Court has not been content only to lop off extreme population malapportionment. It has come close to subordinating all aspects of political representation to one overriding element—absolute equality of population in all legislative districts" (p. 11). Looking at that statement in the light of the New York and Missouri cases of 1969, one can only object that the Court didn't just come close—it actually *did* subordinate all else to absolute equality of population. Dixon's view is that the Court should have followed a substantive due-process argument in the apportionment cases, thereby allowing much more play for legislative judgment but at the same time disallowing the grossest forms of malapportionment.

Bickel expounds his more elliptical position very skillfully. Consider: "From the beginning, the Warren Court's apportionment decisions have consistently asked the wrong questions about American political institutions" (p. 60). And: "The sensible question to ask about any institution of government . . . [is] whether it tends to include or to exclude various groups from influence, and whether, if it assigns disproportionate influence to some groups, they are the ones which are relatively shortchanged elsewhere . . ." (p. 60).

Gordon Baker offers a concise and precise review of the cases, and goes on to consider the mysteries of single-member districts and multimember districts as well as other things. Note Brennan for the Court: "It might well be that . . . a multi-member constituency apportionment scheme . . . would operate to minimize or cancel out the voting strength of racial or political elements of the voting population. When this is demonstrated, it will be time enough to consider whether the system still passes muster" (p. 124–125). That's pretty naive. Any MMD system will badly penalize minorities given only the assumptions that the majority will vote a full and straight ticket. Furthermore, SMDs will badly penalize minorities to the extent that they are spread equally among constituencies or concentrated in a few. At any rate, the Supreme Court backed out of this thicket in the Marion County (Indianapolis) MMD case, saying in effect that minority losses in this situation were just the fortunes of politics and not a matter for the Court. Earlier Court minorities were somewhat amused.

Douglas Rae gives a formal, symbolic exposition of the effect of reapportionment on the norm of equalitarian democracy. Later Milton Cummings sounds a different note from those expressed earlier. He takes reapportionment as reality, and estimates the probable effect on the membership of Congress given existing population distributions. His estimate is that things are not likely to change much but that differences in the constituencies of the President and Congress may decrease.

In an arresting essay, William Bicker looks at the studies of state political system outputs which hold that apportionment really doesn't matter. He argues fairly that these studies are seriously defective since they do not measure crucial variables very well.

In a final essay, David Mayhew writes on theory and practice in drawing congressional districts. The theory part seems to me a remarkable exercise of independence and good judgment. At one point, Mayhew examines various propositions about the desirability of creating competitive districts, including the proposition that congressmen from safe districts behave differently from others. In writing of "the popular myth that electoral insecurity . . . induces congressmen to be 'liberal' . . . " (p. 257), Mayhew contends that "most of the scholarship on competitive and safe congressmen is speculative rather than conclusive" (p. 258). This and more refreshing cold water in a balanced discussion.

Unlike so many collections, this one is a remarkably successful attempt to cover an important issue from various perspectives. Despite its title, however, this book will *not* tell the reader what is going to happen to apportionment in the 'seventies. Crystal balls seem to be a bit cloudy. My own best guess is that President Nixon will continue to reconstruct the Court and that the new Court may be open to the making of distinctions, if not reversals. If that is so, then the questions are how will the legislatures be chosen and how able and determined will they be to change what was forced on them in the 'sixties.

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The Discovery of the Asylum. Social Order and Disorder in the New Republic. By David J. Rothman. (Boston: Little, Brown and Company, 1971. Pp. 376. $12.50.)

Perhaps I can best review this book by attempting to answer three major sorts of queries: What has the author undertaken, and what does he conclude? How well does he succeed, why and why not? And what special interest does the volume hold for political scientists?

Rothman himself begins by asking the question: "Why did Americans in the Jacksonian era suddenly begin to construct and support institutions for deviant and dependent members of the community? Why in the decades after 1820 did they all at once erect penitentiaries for the criminal, asylums for the insane, almshouses for the poor, orphan asylums for homeless children, and reformatories for delinquents?" (p. xiii). He subsequently declares that proper answers must be located in the social attitudes and republican culture of the period, roughly, 1820–1850; and he concludes that legislators, philanthropists, local officials, and others became convinced that the nation faced grave perils from individual instability, family disintegration, and community disorder. "The asylum, they believed, could restore a necessary social balance to the new republic, and at the same time eliminate long-standing problems" (p. xviii).

Rothman, professor of history at Columbia University, succeeds brilliantly on many counts. He has unearthed source materials hitherto unknown or neglected for the early history of public institutions. He has integrated a vast array of demographic, institutional, reportorial, and contextual data into a clearly controlled structure (thematic as well as chronological) for our better understanding. And, especially, he has freshly recaptured the symptomatic vocabulary of an age. Over and over again his aptly chosen quotations contain the imperative watchwords of anxious Americans: order, regularity, system, stability, discipline, industry, social obligation, and obedience.

Rothman succeeds less well when he insists upon excessive contrasts between colonial practices/attitudes and those of Jacksonians toward the socially disinherited. He exaggerates the extent of colonial apathy, and minimizes the real continuity of psychological and physical brutality from the eighteenth century long into the nineteenth. Americans went from barbaric punishments on the books, but sometimes unenforced, to institutionalization that was often barbaric and even absurd. Finally, in overdramatizing an attitudinal shift between the 1790s and 1830s, Rothman ignores some basic economic considerations underlying the colonial tendency toward noninstitutionalization: neither the public fisc nor the private purse could readily afford widescale incarceration.

There are many reasons why political scientists should examine this book. Its two concluding chapters on "The Legacy of Reform" and "The Enduring Institution" are excellent and make vital contact with the penal dilemmas of our own time. The ways in which an earlier generation of Americans tried to make policy and rationalize solutions in response to crime, poverty, alcoholism, and insanity are intrinsically fascinating. Moreover, the traditional sources tapped for the history of political and social thought before the Civil War—Marshall and Taney, Kent and Story, Calhoun and Webster—are in many respects less revealing than are popular attitudes toward family and community, liberty and authority, tradition and change, social reform, and the role of public institutions.

In recent years there has been a tendency among students of antebellum culture to view this period in terms of a psychic "identity crisis." Perhaps there was such a crisis initially, in the years before and following the War of 1812, for example. But the crucial years 1825–1850 might better be characterized in terms of a "quest for society." Most of those heady Jacksonians knew *who* they were, but were much less certain about how they were going to relate to one another. Ultimately, as Rothman helps to demonstrate, the quest for community (or society) failed. Perfectionism and utopianism proved to be passing fancies. The potential for individualism proved stronger than the penchant for collectivism. And, most difficult of all, the Jacksonians' sense of the tension between self and society was strangely skewed, in my view, because they juxtaposed a new and naive, optimistic sense of self with an older, more cynical sense of society: relatively structured, agrarian, and restrictive in its relationships. That incongruous juxtaposition made for misunderstandings then and institutional anomalies thereafter. Many of these incongruities will be discussed by Rothman in a sequel to *Discovery of the Asylum;* it will concern the problems of incarceration in America since the 1890s, and I look forward to its appearance.

MICHAEL KAMMEN

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The Jacksonians Versus the Banks: Politics in the States After the Panic of 1837. By James Roger Sharp. (New York: Columbia University Press, 1972. Pp. 329. $12.50.)

For some time past controversies over Jacksonian democracy have yielded diminishing returns. This book shows that study of politics in the states opens a new field for inquiry and may force the abandonment or revision of several familiar theses. Interest in the Bank question did not die down after Jackson's reelection; it remained the focal point of state politics. In all the states studied, the hard money faction in the party prevailed over the "softs." The dominant ethos of the party was neither rising capitalism nor eastern radicalism.

> The Democratic party did not engage in the battle over banks and currency as the party of the entrepreneur in an age of enterprise. In contrast, the party's appeal was in the nature of a plaintive warning against the increasing commercialization and vulgarization of American life. If the laissez faire logic of the Jacksonian anti-bank and anti-corporate argument eventually led to the enthronement of the business ethic in the last half of the nineteenth century, it is one of history's cruelest paradoxes (p. 321).

Has Sharp gathered enough evidence to support a generalization which demolishes an interpretation that has been widely accepted? Mississippi, Ohio, and Virginia are examined in depth; three other states—Louisiana, Pennsylvania, and New York—are considered more briefly. Some other states are noted in general reviews of the different regions. The author suggests that some of Lee Benson's findings in New York are atypical, but he himself may not have covered enough ground to say what is typical. Some apparent anomalies are not examined at all: Why, for instance, were the Whigs so strong in North Carolina and Tennessee amongst the small farmers who elsewhere provided the backbone for the hard money interest? Or why did New Hampshire stand so firmly as a Democratic bastion in New England, and why was Connecticut a marginal state? However, Sharp can probably give a convincing answer to these criticisms. He has done enough to demonstrate the need to re-examine the whole problem of Jacksonian democracy, and to do so by asking what the Democrats really stood for in the separate states. To have done more at this stage would have postponed (perhaps for years) the presentation of an important argument to fellow scholars. It is important because it has wider implications for the study of American political behavior; for many years political historians have been aware that the Washington story alone is not enough; here is convincing proof that the study of politics in the states may transform the national picture.

A further criticism, which may be more difficult for Sharp to answer, is whether an analysis that depends upon the treatment of a single issue can be adequate. He proves that the bank issue remained vital in the politics of the states he has studied, but it did not stand alone and political behaviour must have been affected also by internal improvements, public and private bankruptcy, public land policy, the tariff, and race relations. How often did one or more of these questions push the Bank question onto the sidelines? Or how often was the popular anti-Bank rhetoric used to promote a less popular aim which was of paramount interest to a group or faction? It is also arguable that the emphasis on state issues has gone too far. Even within his own terms of reference one would expect some examination (particularly in Virginia) of reactions to the abortive Whig attempt to revive the National Bank (as "the Fiscal Corporation") in 1841. A final criticism is that Sharp is sometimes incautious in using political labels. "Conservative" was used by contemporaries, but usually as a term of commendation without modern implications; "radical" was used, but seldom to describe the kind of primitive populism that he associates with hard money; "political broker" is a modern concept which may be useful in analysis but should not be identified as a group with a conscious political purpose. The statement that "at the time of the Panic the Democratic party in the Northeast was firmly in the hands of brokers" (p. 319) does not help one much, particularly when it has to be qualified by saying "this, of course, meant different things in different states." So far from explaining the structure of parties, the incorporation of an anachronistic concept may obscure their character.

Despite these criticisms the book is based on solid research, restores life to a jaded controversy, stimulates further inquiry, and demonstrates the danger of generalizing about the center before one has looked at the periphery.

W. R. Brock

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The Growth and Decadence of Constitutional Government. By James Allen Smith. (Seattle: University of Washington Press, 1972. Pp. 287. $8.50.)

As an intellectual leader of the Progressive movement of the early twentieth century, Professor James Allen Smith was second only to Charles A. Beard. Together, Smith and Beard, perhaps more than any other two men, shaped much of the political thought of the past generation. Considered historically, their chief contribution to the Progressive movement was their explanation of the democratic shortcomings of that era ("the frustration of majority will," "Gilded Age plutocracy," "Tory political practice," "machine politics," etc.) in terms of Constitutional sources. The democratic character of the American Constitution—except for early Abolitionist opposition—had never previously been questioned. In the early twentieth century, however, Smith and Beard were about to launch the first attack. In its initial phase the new approach concentrated on political factors, particularly an examination of the Constitution in terms of the class alignments of its framers. The second phase was economic, an examination of the Constitution in terms of the clash between financial and agrarian interests. James Allen Smith's most widely read and loudly acclaimed book, *The Spirit of American Government* (New York: Macmillan, 1907) was the most authoritative embodiment of the initial, political phase and preceded by six years the publication of Charles A. Beard's *An Economic Interpretation of the Constitution* (New York: Macmillan, 1913), which embodied the second, economic phase.

By 1920, however, James Allen Smith, was "disillusioned and inconspicuous" (p. xvii). Before his death in 1924, he finished the book here being reviewed, *The Growth and Decadence of Constitutional Government,* which dramatizes the depths both of his personal disillusionment and of the dilemma of the liberal reformer in America in any age. The book was first published posthumously in 1930 by Holt, but was almost entirely ignored and sparsely and poorly reviewed. Now, in 1972, it has been reprinted by the University of Washington Press with a concise yet superb new Introduction by Dennis L. Thompson. Before deciding that this book deserved to be reviewed today, I was urged to be convinced that its republication is more than an act of historical piety. I *am* convinced because even though the historical accuracy of the specifics of a Beardian thesis may no longer be central to the concerns of American political science, the problem of political reform within our constitutional framework certainly is. Thompson is correct when he concludes: "The dilemma of constitutional government was the dilemma of J. Allen Smith and is ours as well" (p. xxiv).

The first problem which Smith soon recognized had been posed by the progressive social legislation he had earlier advocated was the inevitability of its centralizing influence, an influence his Jeffersonian sympathies could not tolerate. He had always been willing to admit that although American constitutionalism was essentially countermajoritarian, it had at least accomplished the original objective of liberal, eighteenth-century constitutional thinkers which was not to establish a "democratic government with the people as the recognized source of authority" but to provide a system of restraints surrounding the exercise of *any* type of political authority (pp. 3, 19). But now, he felt, even this had been lost. Not only had progressive reforms (e.g., initiative, referendum, recall) failed to solve America's political and economic problems, and not only had "democracy" (defined in terms of majority rule) been thwarted by continued judicial vetoes and by increased popular subservience to excessive presidential power, but also "individual liberty" was now being sacrificed on an altar of an increasing centralization. In the end, Smith lost his faith in public opinion completely. "Political majorities, no less than public officials," he wrote, "are prone to take into their own hands as much power as circumstances admit" (p. 163). And so he turned (or perhaps returned) in a nearly Aristotelian sense to a distrust of centralization of all sorts, including state encroachment on local government. He claimed that the difficulties with government increase with the size of the governmental unit, that even state governments are purely artificial, arbitrary divisions while the city is the "natural and organic unit with interests peculiarly its own" (p. 193).

It is perhaps the tragedy of James Allen Smith that he could find no adequate means to the noble ends he postulated. Progressive reform, he knew empirically, had largely failed. Public opinion, he finally concluded, was no suitable substitute. And he never recognized that Jeffersonian agrarianism, with its emphasis upon the merits of locality, was an outmoded concept. Possibly Walton Hamilton (one of the few reviewers of this book when it first appeared) was correct when he concluded that Smith's first book was too early and this book was too late (*Yale Law Journal,* November, 1930, pp. 152–53). But I do not think so, because what may have been a personal tragedy to James Allen Smith is simply a professional burden each of us must continue to bear. In the 1920s, the problem was stated to be the task of adjusting a cumbersome governmental machinery to changing popular needs. In the 1970s, the problem might be stated somewhat differently (yet similarly) as the task of adjusting an increasingly technological and socialized governmental apparatus to changing popular demands but still within the context of traditional constitutional principles. Does that mean that we should abandon the instrumentalist view of the state as a mechanism to be used only to obtain ends higher than itself, or must we return, like Smith, to the organic, Aristotelian view that the state is an end in itself, a fellowship in which there can be no outcasts and no inequalities? Finally, what about the fundamental question of popular participation? Smith's principal thesis, you see, was twofold: (1) The Constitution contains undemocratic elements; and (2) it was deliberately devised for undemocratic ends. Although few political thinkers today would quibble about the accuracy of these two statements as empirical observations, they will continue to differ about the proposition of some of the Founding Fathers that the Constitution is *wisely* undemocratic. It remains a task of both empirical investigation and normative speculation to determine whether a perfectly democratic instrument can sustain stable government.

Thus, Smith's tragedy, in the end, is our legacy. I doubt that we will be any less self-contradictory in handling these problems than he was. But we must try, and a good place to begin is with Smith's book. It is a general treatise on political institutions and democratic theory. And although it may not be that classic book about which a reviewer hopes to say, "This need never be done again," it certainly goes a long way in plotting the pitfalls and delineating the dilemmas of constitutional government. I applaud its republication. I hope it receives a wider readership this time around. I think the 1970s in America are a propitious time for a second attempt.

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L'Election Présidentielle des 5 et 19 Décembre 1965. Edited by Gérard Adam et al. (Paris: A. Colin, Centre d'Etude de la Vie Politique Française, Cahiers de la Fondation Nationale des Sciences Politiques, 169, 1970. Pp. 571. $20.00.)

L'Election Présidentielle de Décembre 1965 is part of a continuing series of election studies of the Fondation Nationale des Sciences Politiques. As the title implies, it is concerned with the first presidential election under universal suffrage in the Fifth Republic. It is an immense work, crammed with articles and data of all kinds. There are, for example, chapters on the strategy of the groups involved in the campaign, the role of the unions, the Church, employers' groups and farmers' unions. In addition, at least one third of the book is devoted to a geographical, attitudinal and statistical analysis of the results. If there is something for practically everybody, however, perhaps one of the main defects is that there is simply too much for any one person. Especially in the last 200 pages or so, the plethora of charts, graphs, tables and diagrams makes one wonder whether the passion for detail and definitiveness is not carried to unnecessary lengths. In the future, one would wish for more brutal editing, or perhaps two volumes, one containing the interpretive essays, the second containing the hard data.

The book raises a number of important questions, but one of the most interesting is the extent to which the 1965 French presidential election led to any serious changes in the structure of the French party system. After all, one of De Gaulle's motives in creating the presidential system in the first place was to force the creation of larger and more representative political parties. Yet it would appear that the 1965 election had no real effect on the party system. From Defferre through Barbu, Lecanuet and Marcilhacy, to Tixier-Vignancour, the process by which candidates emerged was more like the imperceptible oozing of toothpaste from a leaky tube than a series of conscious choices made by party structures.

Defferre's ephemeral campaign for example, was the result of inspired journalistic and public relations techniques which finally ran up against the less inspired but much more formidable resistance of the Socialist and MRP party machines. As Collette Ysmal points out in her excellent essay on party strategies, François Mitterrand emerged and then won support as the Left candidate both because of his political acumen and because he was wise enough not to attempt the impossible task Defferre had set himself of trying to reform the very parties whose support he was soliciting for his campaign. Mitterrand shrewdly aimed at uniting the Left party leaders around himself and a common program, nothing more than that. By leaving the party structures untouched he assured himself the support of the party hierarchies. As for the MRP, their nomination of Jean Lecanuet was a defensive gesture against the possibility that a straight fight between Mitterrand and De Gaulle might lose them support on the center. And the remaining minor candidates simply pushed themselves forward onto the political stage.

If the process that led to the 1965 election did little seriously to alter party structures, however, the surprising showing of Mitterrand, who received 32 per cent of the vote on the first ballot, forcing De Gaulle to a second turn, certainly demonstrated the latent strength of the united forces of the Left. But, as François Goguel has pointed out for years now, and as he reiterates in his essay, much of this support rests "less on the existence of factors of an economic or social order, than in the persistence of an old historic tradition, solidly rooted, which leads the majority of the voters in almost a quarter of the French departments . . . to vote for the Left" (p. 407). Equally important, however, is the fact that the 1965 presidential election gave Mitterrand—a man who had never belonged to the Socialist or Communist Left, but to a minor left-wing party and then to a political club—the opportunity to establish himself as a national representative of the Left. When the time came for the old SFIO to reconstitute itself as the Socialist Party, and more recently, when the time came to negotiate a common program between the Socialist and Communist parties, this image, despite the temporary tarnishing during May 1968, was of the utmost importance in attracting the support of the large numbers of traditionally anti-Communist Socialist party members and supporters.

Obviously, it is impossible to discuss any but a few of the themes in a work of this scope and comprehensiveness. It should be mentioned, however, that those who delight in hunting down and then slaying old or new myths will find weapons for their armory here. For example, against those who intuitively assume that farmers would support De Gaulle, there is the article by Marie-Elisabeth Handman and Yves Tavernier to demonstrate that the agricultural unions "intervened directly in the campaign taking a position hostile to General De Gaulle" (p. 286). Or, against those who always assume an important role for the Catholic Church in French politics, one can point out that the Catholic hierarchy scrupulously avoided taking any position at all during the election. The fact, however, that 23 per cent of practicing Catholics voted for De Gaulle, 40 per cent for Lecanuet and only 6 per cent for Mitterrand might prove to some people that indifference is merely a pose when the dice are loaded.

Finally, the ever-shifting nature of French politics is illustrated in the concluding essay by Jean-Luc Parodi's statement that "the direct election of the principal leader of the people . . . has established a new hierarchy which lowers the parliamentary election to second rank" (p. 535). In the light of the absolute indifference that greeted the subsequent presidential election of 1969 and the excitement stirred up by the legislative elections of March, 1973, it is clear that caution should be the watchword of those who make predictions about French politics.

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The Chilean Senate: Internal Distribution of Influence. By Weston R. Agor. (Austin: University of Texas Press, 1971. Pp. 206. $7.00.)

The publishing explosion in Latin American politics in the last ten years has produced some excellent country-studies as well as a number of monographs on the military, the church, and several Latin American political parties. Yet perhaps because of an assumption that conventional legal institutions are almost meaningless in the unstable politics of Latin America, there has been no previous full-length study of its executives, legislatures, or court systems.

The lack of attention to these institutions may be explicable in "banana republics" where military coups and revolutions are regular occurrences, but there are other countries (Chile is certainly one of them, and so in recent years are Colombia, Costa Rica, Uruguay, and Venezuela) in which these institutions are, in fact, institutionalized and play an important and permanent role in national politics. Chile, as demonstrated by the astounding survival of its constitution and political structure in the midst of intense ideological polarization and social conflict, has the most institutionalized political system of all. In fact, Professor Agor reminds us, the Chilean Congress with a history that goes back to the 1830s is the third oldest national legislative body (next to those of England and the United States) in the world. (Sweden and Holland should be added to the list but Chile still ranks in the first five.)

Rather than study the entire Congress, Agor has chosen to focus his attention upon the Chilean Senate. At the time that he conducted his field research, it had only 45 members (since raised to 50), 43 of whom he was able to interview at length. Elected in separate elections from those for the presidency, it was and is a principal center of opposition to the executive. It has a relatively large bureaucratic staff and a complex structure of committees and decision-making bodies, and it operates in accordance with elaborate written and unwritten rules of procedure (including a 755-page Senate Manual) which demonstrate a high degree of institutionalization.

The book uses approaches developed in the study of the American Congress by Matthews, Fenno, Manley, et al. to examine the structures of formal and informal influence in the Senate, to show how it has maintained its independence despite the increasing power of the executive, to describe the internal environment of its specialized committees and interparty conferences, and to delineate the formal and informal processes of socialization which enable it to act effectively in the "management of conflict." Of interest to students of parties and legislatures is the evidence that the Chilean experience contradicts the generalization drawn from the English and American cases that strong parties mean weak legislative committees and vice versa. All parties, especially those on the left, are strong and disciplined in Chile, and yet the Senate appears to have effective and expert specialized committees. Those who wondered in 1970 how an opposition-dominated Congress could have elected Allende in the Congressional runoff for the presidency will find illuminating the description of Allende's success and impartiality as president of the Senate in the late 'sixties—paradoxically, at the very time that he was also serving as an officer of the Latin American Solidarity Organization—OLAS, a Cuban-inspired group dedicated to the promotion of continent-wide revolution.

An epilogue added when the book was in galleys contains the author's initial reactions to Allende's Popular Unity government. It raises questions about the future of civil liberties in Chile as economic pressures on the media make it increasingly difficult for the opposition to express its views. Aside from the epilogue, the rest of the book seems to have been left unchanged from the time that it was written as a dissertation in the late 1960s. Thus in a book published in late 1971, it seems strange to see a reference to the 1968 Salary Readjustment Bill as "a recent example" (p. 8), a description of the Chilean government as one "which leans to the left" (p. 13), the assertion that the Socialists and Communists have "imperfect access" to the bureaucracy (p. 157) and a discussion of the refusal by the Senate to allow President Frei to travel abroad in 1967 which makes no reference to the constitutional amendment adopted at the end of 1969 which permits presidential trips for 15 days without senatorial authorization. The more frequent use since 1970 of the impeachment procedure against cabinet members also modifies the assertion (p. 4) that impeachments have "seldom passed both houses."

Agor twice (pp. 143 and 158) quotes Eduardo Frei's tribute to the importance of the Senate as a training ground for national leadership in Chile, and perhaps the statement bears repeating as another product of many years in the Chilean Senate faces a hostile Senate, and may be tempted as Frei was in 1967, to resort to measures of doubtful constitutionality. To anyone interested in comparative legislatures, political socialization, or simply why the Chileans have been able to pull off their political balancing act in the midst of economic and social troubles that border on chaos, this book is a most useful contribution.

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Indian Political Thinking from Naoroji to Nehru. By A. Appadorai. (Madras: Oxford University Press, 1971. Pp. xxvi, 189. Rs. 10.)

With few exceptions, notably Gandhi's theory of *satyagraha,* the political ideas of modern India have been neglected in the literature on nationalism, on India, and on political thought. Two reasons are noted in this succinct, informative survey by the dean of India's political scientists. First, the principal concern of "thinking men" in 19th-century India was social and cultural reform, not constitutional change. And even after the formation of the Indian National Congress in 1885, attention was riveted to the important but narrow issue of self-government and the methods of attaining it. The second reason is that the national movement and its ideology drew their inspiration from the West: the first generation of nationalists perceived their task as the transplanting to India of those representative institutions and liberties extolled by British liberalism of the age —institutions and liberties which the Moderates accepted as the norm of a "good society." Many in the succeeding generation were attracted to socialism, some to communism, a few to fascism; and all of these ideological persuasions, which moved the vast majority of Indian nationalists until 1920, were acquired through India's connection with the West.

Dr. Appadorai's survey takes the form of capsule summaries of the political ideas of a score of Indian leaders and intellectuals from Naoroji to Nehru. The scope is broad: a dependent people's conception of imperial rule—its merits and defects, and the duties of the colonial power, especially in the sphere of social reform; the case for self-government and the relationship of political to social change; the methods of achieving freedom, from constitutional agitation and voluntary efforts, to individual terrorism, to *satyagraha;* the concepts of nation, state and nationalism; the definition of a good society, including the optimal relationship of state to individual, the role of religion in politics, freedom, economic equality, the equation of means and ends; the conditions of democracy and its compatibility with caste; and the theory of *sarvodaya,* or "people's socialism."

Dr. Appadorai displays a sure grasp of the literature relating to Indian political thinking during the past century. He has succeeded admirably in extracting the essence of views expounded by Indian nationalists, presenting them often in direct quotation, or in a terse, readable style. He has drawn upon half a dozen types of primary sources: essays, for example those by Dayananda, Tagore, Aurobindo, and M. N. Roy; autobiographies by Gandhi, Nehru, Azad, Prasad, and others; works on contemporary issues, such as those by Lajpat Rai, Aurobindo, and Subhas Rose; periodicals, especially the articles by Gandhi in *Young India* and *Harijan,* and by Rajagopalacharia in *Swarajiya;* symposia; and letters—between Gandhi and Jinnah, to and from Nehru, by Sprinivasa Sastri, Golwalkar, and others. The bibliography will be helpful to students, as will the brief biographical sketches and glossary of terms.

Among the early nationalists and publicists the greatest attention is given to Naoroji, Gokhale, R. C. Dutt, Tilak, and Surendranath Banerjea, with occasional references to Ranade, Pherozeshah Mehta, and less well-known figures, such as Sayani and in addition Bomanji, who in 1863 urged limited British immigration to India to stabilize imperial rule (!). Gandhi's ideas, especially those relating to political methods, are treated at some length, as are the views of Aurobindo on religion and politics, and Tagore on nationalism. And among the contemporary figures, Nehru, Vinoba Bhave, and Jayaprakash are discussed and the fundamental ideological cleavage between Gandhi and Nehru is laid bare.

The survey also contains, though much too briefly, summaries of uniquely Indian ideas on politics—Dayananda's essay on "The Science of Government," Vivekananda's theory of political cycles, and Narayan's concept of communitarian democracy. The last deserves more attention in the West. While acknowledging the heavy debt to Western thought (in Aurobindo's words, "servile fidelity"), the author discerns some "fruitful ideas" deriving from Indian values and experience: the notion that imperial rule, no less than self-government, must rest on consent, not on force; the view that self-government is not only preferable but necessary to good government; the principle that desirable ends can be attained only through right means; the aim of "welfare for all" instead of the utilitarian maxim; the limitation of material wants as necessary for a good society; the doctrine of trusteeship; *satyagraha;* and the *sarvodaya* on political and economic decentralization. Most of these, however, as the author notes, have not yet been developed sufficiently to be regarded as contributions to political thought.

This is not an original book, nor was it intended to be. It can serve, however, as a very useful introduction for Western students, to the political thought of a dependent people during its struggle for independence. It also reveals the extent to which Indian nationalism was influenced by the West in articulating its claims to freedom. The distinctively Indian dimension of those ideas emerged late in the struggle and was dominated by Gandhi, with Aurobindo, Tagore, Golwalkar, and others on the periphery. This book would have been more valuable had greater attention been given to these *Indian* ideas. Nevertheless, it will be a handy guide to Appadorai's *Documents on Indian Political Thought 1857–1964.*

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Peasants Against Politics: Rural Organization in Brittany, 1911–1967. By Suzanne Berger. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 298. $11.75.)

In the social sciences, case studies are a difficult art, because of their contradictory demands. Suzanne Berger must have known this in heading for Landerneau (Finistère) to study the powerful agricultural organizations of that modest rural metropolis. Inspired both by the questions of a political scientist and by the taste for the unusual that motivates an anthropologist, she spent a year in a Breton "tribe" that must have seemed very strange to a Harvard student. To this surprising choice we owe a masterpiece that demonstrates remarkable competence, reflecting an intimate knowledge of French and Breton affairs as well as sound scientific procedure, a rare capacity to pull out of a unique situation a theoretical lesson of general importance, and a talent for writing that makes the book enjoyable to read.

"Landerneau": the word sums up almost a century of the political and agricultural history of France. Suzanne Berger tells the story in linking local events to national decisions. The refusal of the royalist and Catholic nobles to compromise with the Republic led them to shut themselves up in their chateaux and to take up caring for "their peasants," then to fall to squabbling when some, heeding the Pope's words, decide to betray the cause and to enter the system; for this deed Albert de Mun lost his seat as a deputy. Soon the conflict shifted to one between monarchists won over to the Republic by realism but not conviction and those Catholics who were genuinely republicans: the Sillon movement resonated powerfully and aroused the outrage of the traditionalists. Between the World Wars, the Socialists appeared on the scene, and the rivalry between the conservative Vicomte de Guebriant and the Socialist Tanguy-Prigent expresses in microcosm both local and national history: the Popular Front; the Peasant Corporation of which Guebriant was national syndic; the Liberation, when Tanguy-Prigent became de Gaulle's Minister of Agriculture and founded the Confedération Générale d'Agriculture; the Fifth Republic, when a new generation of young "revolutionaries" finally appears, men more concerned with economic organization than with political games who by violent action succeed in imposing their will on the government and on other producers. What a long road lies between the Breton nobles coming to pay homage to the Count of Paris, the royalist pretender at Jersey in 1887 and the battle of the artichokes in 1961: economics instead of politics; values of efficiency instead of moral values; mass action in place of elite intervention; the defense of successful farmers instead of the defense of all the rural world. At the end of this long road, Landerneau consults with young economists who are Marxists and technocrats from the Ministry of Agriculture. It is as if no longer knowing which saints to pray to, the heirs of the Vicomte have turned to the devil.

The principal mechanisms of this slow evolution which ends in a brutal revolution are meticulously described by Suzanne Berger, who also emphasizes the permanence of certain structures. In the nineteenth century, this peasant society lived completely on the margins of the French nation to which it was linked only by the mediation of several powerful noble families. The Church was the only institution that included the village and went beyond it. Thus Brittany was monarchist even though the Bretons were not; the handful of nobles who went to greet the Count of Paris represented all there was of political life, and the fight between those won over to the Republic and the faithful was a battle fought out only in the salons of the chateaux. The Sillon, on the other hand, took root in the rectories. A national institution created by the Republic, if not Socialist (like the Office of Wheat in 1936) could become the best reinforcement of Landerneau's authority by allowing this agricultural cooperative to appropriate state powers on the local level, in sum by allowing it to carry out the functions that notables play in peasant societies. This is indeed one of the characteristics of the exercise of power in France:

> Each group strives to be sovereign in its own domain and to reduce as much as possible the overlap of functions and the sharing of power that characterize decentralized political systems. It is not difficult to understand why many Frenchmen fear decentralization as a prelude to the disintegration of the state, for, until now, whatever powers autonomous groups have acquired they have tried to exercise beyond the reach and influence of the state (p. 9).

In this isolated peasant society, political life penetrated through the Church—through it and its internal quarrels, and against it when anticlerical republican notables appear on the scene after the separation of church and state. It is therefore not surprising that today the church and politics are still inextricably linked, for where the grandparents' generation saw one through the lens of the other, the children's generation see the two as the same.

All this would not account for the differences between Cotes-du-Nord and Finistère, which are analyzed in depth by Suzanne Berger, who asks why two neighboring rural societies that are virtually identical should have chosen two different paths to integration in national society: syndical organizations in Finistère, political parties in Cotes-du-Nord. When all is said and done, the only explanation appears to be historical, which of course, simply pushes the explanation further back in time:

The emergence of the corporative group in Finistère and of the political part in Côtes-du-Nord as the dominant modes of linkage of the peasant to the nation depended on the decisions of elite groups, and their choices between the two organizational models were determined by combinations of circumstance, personality, and preference, and not by the press of economic, social, or political factors. . . . Here, as for special movements that have been studied in other countries, the question appears to be which one of a number of "possible" groups manages to organize the peasants first" (p. 161–2).

These examples show that Suzanne Berger has used the case of Landerneau to ask questions so penetrating and so illuminating that her book makes important contributions to the understanding of French society, to the theory of the peasantry, and to political science. It is a fair wager that it will rapidly become a classic in each of these three areas.

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The Theory of Moral Incentives in Cuba. By Roberto M. Bernardo. Introduction by Irving Louis Horowitz. (University: The University of Alabama Press, 1971. Pp. xix, 159. $7.50.)

Revolutionary Change in Cuba. Edited by Carmelo Mesa-Lago. (Pittsburgh: Pittsburgh University Press, 1971. Pp. xvi, 544. $14.95.)

These volumes are a welcome addition to the literature on the Cuban revolution. The period of polemics is not over, but it must make way for more scholarly analysis. These two books are part of the new day.

The Mesa-Lago volume is a comprehensive treatment, covering not only the political and economic aspects of the revolution, but also the changes in the social structure and the institutions of education and religion, along with the theatre and literature. Besides the editor, there are sixteen contributors, each an acknowledged authority in his field. Mesa-Lago, himself one of the leading authorities on Cuba, is the author of two very important chapters, and joint author of three others. The book, in general, must be considered the most comprehensive and authoritative coverage of the revolution that has appeared to date. Books such as these are laying solid foundations for further works.

In the opening chapter, "Leadership, Ideology, and Political Party," Professor Andrés Suárez presents indispensable background information for the subject matter that follows. He rightly emphasizes the central importance of Castro as the charismatic leader throughout the Revolution, and minimizes the role of ideology and the political party. Castro, he says is not an ideologist and has shown little interest in it. Moreover, he has forestalled any tendency toward institutionalization of the Revolution. The Communist Party of Cuba, Suárez notes, fails to meet a crucial test laid down by Lenin as being unrepresentative of workers and peasants.

James Malloy, an authority on the Bolivian revolution of 1950, sees the Cuban revolution as a "modern developmental" one, defined as "an attempt by means of revolution to pull a society out of economic backwardness and set it on a course of rapid economic development" (p. 23). He accepts the Cuban revolutionaries as having "successfully erected . . . a new order . . . out of fragments of the old . . . and with such speed and relative lack of bloodletting that the Cuban experience stands in stark contrast to all other modern revolutions, which went through long bloody epochs . . ." (p. 23). One must agree that a "new order" has replaced the old in Cuba, but question the permanence of the "replacement," as long as the country is unable to support itself.

Cole Blasier, an ex-foreign service officer, and now Professor of Political Science at Pittsburgh, in the chapter on "The Elimination of United States Influence," rightly considers "The Cuban-United States break . . . the single most politically significant aspect of the Cuban Revolution" (p. 43). The chapter presents an excellent review of the salient events that led up to the tragic severance of relations in January, 1961.

This break in Cuba's relations with the United States, as Blasier remarks, left Castro at the mercy of the Communists. "Relationship with the Soviet Union" is expertly described and analyzed by Professor Edwardo Gonzalez in Chapter 4. These sometimes rocky relations, conflicting policies vis-à-vis other Latin American countries are well described. A further elaboration is contained in Chapter 5, "Exporting Revolution to Latin America," by Ernesto F. Betancourt. Stressing the differences between Cuba and other Latin American countries. Betancourt presents convincing reasons why the strategy of export has failed.

The first major section of the volume ends with a provocative essay by Professor Irving Louis Horowitz, "The Political Sociology of Cuban Communism." His theme is the "Stalinization of the Cuban Revolution." Stalinization is defined essentially as involving "emergence of a leader and his small coterie as exclusive spokesmen for the Communist party" (p. 127). Horowitz suggests that Cuba is entering a "new stage" which simplifies the "sociological problem" because it is necessary only "'to pay attention to what the leader says and having it stand for what the nation ostensibly believes" (p. 128). This hardly seems a new stage for Cuba, although there was apparently more freedom of discussion when Guevara was on the scene. But all will agree surely that there is only one spokesman for the Revolution.

The section on "Economy" opens with an able exposition of the evolution of central planning, including the choice made among three alternative methods, and the organizational structure designed to implement it. Mesa-Lago, with the assistance of Luc Zepherin, has put together what is undoubtedly the best existing description of this crucial aspect of the Revolution. Cuban planners, the authors conclude, are still experimenting, not unmindful, perhaps, that Castro himself has complained of too much centralization of planning and administration. The authors note a serious failure to allow grass-roots participation in the planning process: "Decisions are made at the top of the hierarchy . . . and the function of trade unions and farmer associations is mainly to endorse the plan and mobilize their membership for its implementation" (p. 180).

In Chapter 8, Professor Roberto M. Bernardo of the University of Guelph, discussed "Managing and Financing the Firm." His book, listed at the top of this review, will be treated below. In this essay he describes and illustrates with charts the structure of both the nonagricultural and the agricultural firm. The Revolution grouped all enterprises engaged in producing the same or similar items into *consolidados,* and the agricultural enterprises into *agrupaciones*. These are the subject of Bernardo's discussion.

With Roberto E. Hernández, Mesa-Lago also shares authorship of an important chapter on "Labor Organization and Wages." Leading authorities on this topic, the authors ably recount the destruction of the free labor movement and the subsequent employment of the organization by the regime as a "transmission belt" to implement its decrees. In short, the Union leaders no longer concern themselves with the welfare and interests of the workers, but rather with enforcing labor discipline in the interest of increased production.

The serious condition of Cuba's international trade imbalance is presented in Chapter 10 by Eric N. Baklanoff. Making judicious and effective use of available statistics, the author contrasts the more favorable conditions that existed in the 1950s with the increasingly unfavorable trends under the Revolution.

Mesa-Lago in Chapter 11 contributes another outstanding discussion, this time on "Economic Policies and Growth." He concludes that although the economy has steadily "deteriorated" since 1961, production should be expected to increase in agriculture with the utilization of larger areas because of land-clearning, increased mechanization, and use of fertilization and irrigation.

Nelson Amaro and Mesa-Lago in their essay, "Inequality and Classes," summarize the effect of the Revolution on the social structure of Cuba. The authors note that old differentials between rural and urban sectors have been reduced, overt discrimination against blacks abolished, although subtle racial differences remain. The wealthy class has been removed from the scene through liquidation, exile and expropriation of property. The lower classes, on the other hand, have had their status improved. The net result has been a trend toward egalitarianism.

What many observers regard as the most important achievement of the Revolution is in the field of education. Professor Roland G. Paulston in Chapter 13 reviews briefly the history of education under the Republic and gives a good exposition of what he terms "Innovations in Cuban Education Under the Revolution," These developments include the construction or renovation of numerous schoolrooms, the impressive gains in enrollment particularly in the primary grades, the literacy campaign, and the establishment of a pervasive program of adult education. Boarding schools which provided food, clothing, shelter, medical care and regular instruction, brought thousands of rural children into the schools.

Mateo Jover Marimón in Chapter 14 reviews the history of organized religion under the Revolution, including the early conflicts and the final development of a modus vivendi. Surely one of the best expositions of this problem.

This reviewer is not competent to give any judgment on these chapters: "Theatre and Cinematography" by Julio Matos; and "Literature and Society," by Lourdes Casel.

The essay by José A. Moreno, "From Traditional to Modern Values," should be read in connection with Chapter 12 on "'Inequality and

Classes," which it illuminates by describing the modifications in values associated with the trend toward equality and away from "hierarchical elitsm."

In the concluding chapter of the volume, Mesa-Lago first summarizes the present situation of the Revolution, then speculates on the future.

In *The Theory of Moral Incentives in Cuba,* Professor Bernardo has selected a limited but highly important aspect of the Revolution for in-depth analysis. As I see the work, it seeks to answer three main questions: (1) Why did the Cuban leaders finally adopt the moral incentives system? (2) What have been the consequences of its adoption? (3) What is the outlook for the future?

In a well-documented account of the "great debate" during the "formative years" of the Revolution, Bernardo argues convincingly "that the leading Cuban revolutionary intellectuals possessed a classical (anti-market) ideology which influenced the choice of organizational means" (p. 3). The debate was between Ernesto "Che" Guevara and his followers and those who wanted something like "market socialism," something midway between capitalism and communism. But, the Guevarists won out, not because the alternative systems could not achieve the desired goals of development and a more egalitarian society, but because of the ideological commitment to classical Marxism.

The second question on how the system has worked up to around 1970 cannot be answered with precision because of the paucity of data. Bernardo attempts to find some usable estimates of the trend of the national income (GNP) over the years since 1958, but with little success. He submits, tentatively, a hypothesis of the variations from year to year based upon what he could find (p. 115); the net trend seems to be a horizontal or even downward line. He lists the sugar production by years, but despite the harvest of 8.5 million tons in 1970, the average annual production showed little change from the pre-revolutionary years. If the author could have added the 1971 crop of under 6 million tons (the target was 7 million) and the crop of 1972 (which the regime has failed to report, but which other sources estimate at a disastrous level under 4 million tons) he would have had to report an even poorer average.

The third point on the possible future of the system can only be answered speculatively, of course. The author suggests for one thing that the "heavy reliance of Cuba on international trade . . . will call for market-minded experiments . . ." (p. 128). Since 1963, the Cuban leaders have sought to achieve economic development through purchase of needed machines and other items from abroad, paying for them primarily through the exports from agricultural production. Since agriculture has not proved an adequate source of exports up to now, the author seems to think the government will face the necessity of making some compromises. Another factor which may prove important is what he calls the "'ideological erosion stemming from the ideological transformation in Eastern Europe" (p. 133).

Bernardo says nothing about the possible dictation by the Soviets of a change which they are in position to insist upon and which would result in greater production than the moral incentive system. For Cuba is increasingly costly to the socialist countries, who may regard the ideology of Cuba as a luxury they can ill afford.

This work is the most extensive treatment of what is and has been a crucial factor in the Revolution up to now. But one suspects that Bernardo would be among the first to disclaim it as "definitive" as the book jacket asserts.

LOWRY NELSON

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Organized Complexity: Empirical Theories of Political Development. By Ronald D. Brunner and Garry D. Brewer. Introduction by Harold D. Lasswell. (New York: Free Press, 1971. Pp. xvii, 190. $7.00.)

Considerable time has passed since the profession has seen the appearance of a book unabashedly proclaiming the merits of a particular methodological approach, especially since the advent of what some call the postbehavioral era. Ronald D. Brunner and Garry D. Brewer have written such a book, and have done so with brilliant competence and verve. *Organized Complexity* even evokes a nostalgic twinge for an earlier time when battles were fought over methodologies. The authors' enthusiasm for the approach they are advocating in fact interjects one of the only weaknesses in what is otherwise the most important book yet written on the uses of computer simulation for the analysis of political development. Their desire to talk to the general subject of how a computer simulation model can advance our ability to understand *complex systems* on occasion diverts their attention from political development and specifically from the empirical settings—Turkey and the Philippines—from which the model was created.

The book is organized as two distinctly separate essays, although the model around which the first is built serves as the basis in the second essay for discussing how computer simulation can be put to use to solve problems found in fields having scientific, historical, normative, and policy implications, as well as in such empirical approaches as cybernetics, structural-functionalism, and political culture. Their demonstrations of how computer simulation might be applied to solve problems in these fields are uniformly ingenious, sophisticated, rigorous, convincing. But while they are all these things, the demonstrations are not always germane to the themes of the first essay—modernization and mass politics—despite the fact that each draws upon the model and its Philippine and Turkish applications. The reason for this deficiency is that the authors' primary concern in the second essay is with making a convincing case for their methodological package, not with subjecting their model to critical analysis nor with applying the insights gained from their work as a basis for a probing discussion of the theories—especially political development—to which they would apply computer simulation.

The fundamental proposition underlying both essays is that political systems are complex and that the complexity is not random but organized. The authors build on a position advanced in the scientific community in the late 1940s that simple systems characterized by a few variables are readily amenable to mathematical analysis and that problems of disorganized complexity can be handled by statistical analysis but that structured systems having a large number of interrelated variables demand some alternative approach. They believe that the use of computer simulation in the form they have developed it provides the best method available for dealing with the analytical problems arising from the study of complex systems—or organized complexity—of which political communities are an example.

Their introduction of the actual modeling process is made through a gentle overview of the weaknesses of Daniel Lerner's analysis of the modernization process, weaknesses they see as due largely to his use of regression analysis to delineate *patterns* from cross-national data. They would have developmentalists concern themselves with *processes* of change, and with underlying structures. They share with Lerner recognition of the importance of urbanization to modernization, but insist, in contrast to his position, that ". . . urbanization need not be treated as an uncaused cause, but rather may be considered in large part a function of relative economic performance in rural and urban areas" (p. 10).

After this introduction, the authors proceed to construct a model containing a set of relationships they believe central for defining the structures and for accounting for the processes of change taking place through time that roughly approximate their understanding of two closely similar empirical political systems—the Philippines and Turkey during the 1950s. The model is built with considerable thought and infinitely careful tinkering. On the basis of an examination of relevant theoretical literature on political and economic development and of case materials from the two nations, they proceed to stipulate the relationships they believe central to the interactions of the rural and urban sectors of the countries and of three subsystems within each sector (demographic, economic, and political).

The complexity of the working model they construct defies verbal description in a short review. In brief outline, however, the model is divided into two-sectors (rural and urban) and incorporates 22 variables and 20 parameters; 12 basic relationships are spelled out in equations governing the model's performance. By way of example, one of these relationships is given its verbal description in this manner: "Expenditures by the government in each sector vary with the number of people in each sector who support the government, the change in the number of supporters in each sector, and with total government revenues" (p. 20). After the initial conditions for the variables have been established and the values of the parameters set, and after "fine tuning" has gotten the ten-year generated runs to approximate the historical time-series, then the model's two reference sets are ready for use. The authors' primary application of the model to the two sets of inputs was to explore the public policy implications of small changes in one or more of the parameters on system performance, e.g., whether birth control policies had significant effects on urbanization rates, or what effects austerity programs in one sector have on living standards and on support for the government. As mentioned earlier, the entire second essay is devoted to discussion and illustrative application of computer simulation modeling to various other fields of political science.

Two weaknesses of the specific simulation-modeling enterprise as applied to the Philippines and Turkey deserve comment. Since the model is intimately linked with empirical conditions in the two nations, the theoretical constructs and the types and quality of case materials utilized in constructing the model are of strategic significance. An examination of the case materials used for one of the nations—the Philippines—shows that the authors relied on an extremely limited quantitative and qualitative range of description about how the Philippine political and economic systems operate. While their grasp of the theoretical literature seems to permit them considerable room for creativity, the conventional orientation of that literature minimizes the importance of outside influences on the political development process, and thereby reinforces a similar bias in the case materials. The result is that the model contains no provision for inputs into the system from outside on any basis comparable to the 12 relationships built into the model (provision was originally made to have one outside input "read in [-to the model] as data" [p. 19], but lack of Philippine data and a decision to drop the variable for Turkey removed even this limited recognition of the larger environment). With one of the two economies basically dominated by outsiders, with each of the nations' military and paramilitary systems heavily supported from outside, with sizable loans and grants flowing (quite often irregularly) in, and increasingly sizable capital flows (profit remittances; debt management payments) moving out, the decision to construct a model based only on indigenous variables and parameters should, at a minimum, have been defended openly and not developed as if no other theoretical alternative were possible.

The second weakness is lack of data. The authors have displayed considerable ingenuity in generating their own data. For example, out of the 16 initial conditions for the Turkish model, only two are derived from direct measurement. All others were arrived at by intuitively disaggregating national figures into the two sectors. In the case of parameters, none could be derived from independent sources, all had to be generated through trial and error during the early stages of model construction. The result obviously is that the model's performance cannot be judged a successful or an unsuccessful depiction of the structures and processes modelled until independent measures of the variables and parameters have been found. The authors are perfectly aware of this weakness and are careful to make clear the limits imposed by the data problem.

Despite these reservations, *Organized Complexity* is a masterfully executed argument for the use of computer simulation in building empirical theories of political development and for its more general use throughout the profession. It is also much more than that: it is an important substantive contribution to the theory of political development.

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Revolution in Central Europe, 1918–1919. By F. L. Carsten. (Berkeley and Los Angeles: University of California Press, 1972. Pp. 360. $11.95.)

In his latest study Prof. F. L. Carsten surveys the upheavals which attended the close of World War I in Central Europe, their immediate antecedents, their successes, and their failures.

Revolution in Central Europe is primarily concerned with events in Germany and German-Austria; there are digressions at appropriate points on Hungary and Czechoslovakia, but these, while perfectly sound, are brief and do not add much to our knowledge of events in those countries. They are largely based on secondary sources in Western languages; the pages on Germany and Austria are based in good part on archival materials.

The heart of Carsten's book—Chapters 2 through 7—deals with workers', soldiers', and peasants' councils in Germany and Austria, and with the emergent Austrian home defense unit, the *Volkswehr*. Particularly valuable is the extensive coverage of events in the provincial cities and the countryside, as well as in the capitals. Succeeding chapters are concerned with organizations of the extreme right and extreme left: *Freikorps* units, Communist and left Social-Democratic parties. Scholars in search of a consecutive narrative of the events of 1918–19 will be disappointed; Carsten's focus is not on the policies of the governments of the day or the overall diplomatic picture but rather on the various extragovernmental groupings which shared or disputed power with the recognized authorities. Nor will political scientists find here any attempt at a comparative analysis of events; Carsten is content to trace the course of developments in Germany and Austria without undertaking to set up any political or sociological framework for comparison.

Revolution in Central Europe concludes with chapters documenting frontier conflicts with Poles and Yugoslavs, and the inevitable reaction to the upheavals of 1918–19—the drift to the right and the eventual failure of the revolutions. Carsten's conclusions are somber, but there are few today who would dispute them.

The councils in Germany and Austria were not revolutionary but auxiliary institutions, which willingly yielded up their authority to their respective National Assemblies when their task was completed; dissension between Communists and Social-Democrats hobbled the Left and opened the door to the Right; nationalization of industry and democratization of society were strictly limited, while the powers of the old regime—the old civil service, judiciary, and General Staff—largely remained or returned; reform affected only the political and constitutional sphere, not the social and economic structure, creating the conditions which were to prove responsible for the weakness of interwar democracy.

Revolution in Central Europe is a lucid, balanced, factual, and thorough study of popular councils, paramilitary organizations, and extreme parties in the German and Austrian revolutions of 1918–19. Political scientists will find in it a wealth of empirical data, from a wide range of primary and secondary sources, on those several sorts of groups, though they will still have to go elsewhere for a comprehensive account of the post–World War I revolutions in Central Europe.

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Changing Latin America: New Interpretations of Its Politics and Society. Edited by Douglas A. Chalmers. (New York: The Academy of Political Science, Columbia University, Vol. 30, No. 4, 1972. Pp. 193. $3.50, paper.)

This volume is an outgrowth of papers presented at a conference sponsored by the Academy of Political Science in May, 1972. The goal of the conference was to present "a part of the effort to find a more realistic view of this vast region" (p. ix), and to replace the enthusiastic simplifications of the early 1960s with "a subtler, more realistic appreciation of the complex and profound forces shaping Latin America's future . . ." (p. x). The pursuit of this goal consists of thirteen readable and interesting essays by a judicious combination of academicians and members of governmental and international agencies.

The essays in the book, far-reaching in scope, can be divided in two groups. The first set discusses new challenges for policy and politics. Included here are articles on the areas of major reforms of the 1960s—agrarian reform (Carter), education (Meyers), and population (Viel). Two potential agents of both change and constraint are examined—urban migrants (Cornelius) and radical priests (Vallier). Arturo Valenzuela offers an interesting analysis of the political constraints in Chile that create a pronounced incongruity between the ideology of socialist planning and the country's incremental bargaining system, the end result being a "politics of conciliation." Finally, Philippe Schmitter trenchantly analyzes previous models of Latin American political development, but goes on to argue for future paradigms that can deal more effectively with Latin American diversity. Contending that "several, if not most, of the Latin American polities are 'spaced-out' along the corporatist-authoritarian path" (p. 94), Schmitter suggests that future investigations explore "three clusters of allegedly determining factors, the Ibero-Mediterranean ethos, delayed development, and external dependence" (p. 95).

The second set of essays deals with the changing bases for Inter-American relations, particularly Latin America's relation to the United States. Topics covered here include Chalmers's critique of "developmentalism" as an explanatory concept for Latin politics, scholarly exchanges between Latin America and the United States (Wood), the increasing importance of relationships among Latin American countries (Agor and Suarez), technology transfer (Morgan), Latin American diversity and United States foreign policy (Bronheim), and the implications for Inter-American relations in the post–Alliance for Progress period (Levinson).

As might be expected, this work, like most collections of essays with a very eclectic orientation, appears frustratingly incomplete and uneven in its coverage. Although only a partial effort is claimed on behalf of the participants in the symposium, one cannot help but question the logic of including full-length articles on urban migrants and clerics as agents of change or constraint, and not providing for a similar article on the military. In Brazil, a conservative, modernizing military regime has effectively coped with a staggering inflation, although it has done so by using political repression and neglecting the thorny problem of income distribution. General Velasco's revolutionary military government in Peru has embarked upon many major social reforms, while Omar Torrijo's highly nationalistic military government in Panama has consistently been at loggerheads with the United States over the future of the Panama Canal. With these and other examples of military "Nasserism" or popularly based dictatorship, one wonders if the neglect of this topic is not a major omission. Although articles on other topics occasionally mention the military, the absence of more complete coverage mars the symposium's goal of "a subtler, more realistic appreciation of the complex and profound forces shaping Latin America's future. . . ."

Furthermore, this endeavor stumbles in its effort to provide new interpretations of Latin American politics and society. Much of the material criticizes other concepts without suggesting alternatives (as does the editor's own article on "development" and "developmentalism"). It repeatedly emphasizes the diversity within Latin America, and stresses a need to examine changes in fundamental assumptions underlying the United States government's attitudes toward the area. While all of this material is in many ways useful, it does not provide us with the alternative conceptual foci necessary for a more sensitive and realistic understanding of the complexities of the region. An exception is Schmitter's imaginative essay on paths to political development, for it goes beyond critiques of past conceptual efforts to formulate some alternative possibilities.

The articles by such reputable scholars as Vallier and Cornelius are insightful, the former for its typological construction of clerical radicalism and pastoral radicalism and the latter for its synthesis of available data on political attitudes of urban migrants. They are, however, basically syntheses with minor elaboration of material more fully developed elsewhere. These interpretations might be new to some readers but certainly not to individuals working in the area.

Overall, this volume constitutes an adequate but hardly exceptional contribution to a field in which the necessary new interpretations have only begun to appear. It does compare favorably with other existing essay collections. The more noteworthy essays, in my judgment, are those by Schmitter and Valenzuela. The primary value of this undertaking would be for assigned or outside reading in undergraduate classes and introductory graduate seminars.

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Korea: The Third Republic. By Kyung Cho Chung. (New York: The Macmillan Co., 1971. Pp. xii, 269. $6.95.)

One of the hazards of studying and writing about societies in flux is the relative rapidity with which the fruit of one's painstaking labor becomes obsolete. Herein lies the key to the plight of Kyung Cho Chung's third book on Korea: No sooner had it been published than it was overtaken by events. The "Third Republic," the subject as well as the *raison d'être* of the book, abruptly disappeared from the scene, being succeeded by the "Fourth Republic" whose constitutional order bears only a faint resemblance to that of its predecessor.

The purpose of his book, Chung writes, is "to present a complete study of the Government of the Third Republic and its political development and an analysis of the critical problem of Korean reunification . . ." (p. x). Although the author does provide ample information on the constitutional-legal framework of the Third Republic, its institutions and standard operating procedures, and the principal events that befell it, it is debatable whether his is indeed a "complete study."

To a political scientist, a major drawback of the study appears to be its preponderantly descriptive orientation. Not only is the book barren of any theoretical concern, either explicit or implicit, but it also lacks any coherent thesis. More than anything else, the book is at once a verbalized organization chart, an annotated constitution, and a journalistic chronicle of the Third Republic. For the experienced student of Korean politics, it offers very little that is new; for the layman it probably provides more factual details than he would ever need or care to know. But, then, the study may not have been intended as a scholarly work. Indeed, there is not a single footnote in the entire volume.

What makes this lacuna disturbing, however, is that the author has incorporated in his book not simply the ideas but the *words* of other people without specifically revealing their sources. That is to say, he quotes without really quoting. To be sure, he does acknowledge at the outset his indebtedness to "authors, publishers, . . . for permission to quote . . ." (p. xi). And he even mentions specific individuals who have "allowed [him] to quote from their valuable publications" (p. xii). Nevertheless, in the main part of the book, one finds passages from other sources with neither quotation marks nor footnotes—a practice which is certain to be frowned upon by the affected authors.

If there are any parts of the book which still remain relevant notwithstanding, or, perhaps, because of, recent upheavals in Korea, they are the two final chapters dealing with democracy and reunification, respectively. In the chapter on democracy, Chung briefly dwells on the possibility that "South Korea's new elite of politically favored businessmen may attempt to lengthen the tenure of President Park to make him a life-time president, by opening the door officially for the fourth term" (p. 190). Underscoring the need for an electoral transfer of power, he cautions that, should such a transfer fail to materialize "within a reasonable period of time, public discontent may reach the boiling point, leading to popular demonstrations, coup attempts or revolutions" (p. 191). "These," he continues, "may be momentarily controlled by increasingly repressive government actions, but eventually a political chaos may arise and the president may be deposed by some nonelectoral means" (p. 191). In view of the traumatic events in South Korea since October, 1972—the declaration of martial law, the drastic curtailment of political liberties, and the extension *ad infinitum* of the tenure and powers of President Park in the name of "revitalizing reforms"—one wonders if the pattern postulated by Chŭng will indeed be set in motion.

In the final chapter of the book, Chung argues passionately that reunification is the only remedy for Korea's chronic problems, exhorting "North and South Korean authorities" to "put forward a proposal to meet at Panmunjom for a transitional step to conduct correspondence, travel, commerce and cultural intercourse between the North and the South" (p. 227). In August, 1971, the two Koreas reached a dramatic agreement to initiate a dialogue at Panmunjom through their respective Red Cross organizations. This historic step was followed by top-level secret political contacts culminating in the publication of a joint North-South communiqué on July 4, 1972. In short, "North and South Korean authorities" have been meeting periodically not only at Panmunjom but also in Pyongyang and Seoul since late 1971. If such contacts have lessened tensions in the Korean peninsula, they have yet to produce tangible results that would pave the way toward reunification.

Somewhat enigmatic is Chung's assertion that "a united Korea is foreseeable only when the great powers and the United Nations will exercise the principles laid down in its Charter" (p. 228). For the greatest impediment to Korean unity appears to be neither the great powers nor the United Nations but the vested interests of the power elites in the two Koreas. In his very last sentence of the book, Chung expresses the hope that "the fourth Republic of Korea will be a 'unified, independent, and democratic' 'Land of the Morning Calm'" (p. 228). What a shame it is that the "Fourth Republic" has thrust itself upon the scene so soon and that it can flaunt neither unity nor independence nor democracy!

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Centre-State Relations Since 1967: An Interim Assessment. By Ramesh Dube. (Delhi: Kanpur Printing Press, 1971. Pp. 96. No price given.)

The author attempts to study the functioning of the Indian federal system since 1967. Dr. Dube points out that "the expression 'Centre-State relationship' has often come to connote the attitudes, actions and interaction of the parties at the Centre and in the States" (p. 2); "The idea of a 'State' within a 'State' is a concept which is becoming familiar to us now" (p. 10). In general, the right-wing parties—the Jan Sangh and the Swatantra—wanted a strong Centre so as to prevent Communist States from going too far, while the two Communist Parties were afraid of a sharp swing to the right in New Delhi, and wanted to limit the possibility of interference by the Centre particularly in West Bengal and Kerala, where they were dominant partners in non-Congress coalition governments. But what complicated further the Centre-State relations was the fact that the coalition politics undermined the powers of the Chief-Ministers of West Bengal, Kerala, and Uttar Pradesh, and this made difficult the settlement of any dispute between the Centre and these States. Although the Centre tried hard to rise above party considerations and conduct itself as the national government rather than as a party, the Centre was unable to evolve healthy conventions to guide its relations with the States; in particular its handling of the State Governor's discretionary power in dismissing non-Congress governments was often partisan.

Finding the confrontation between the Centre and the States "a matter of serious concern" (p. 18), the author argues that the Indian federation is a compromise between a purely federal structure and a unitary government with overriding powers in most spheres allocated to the Centre; it is "a functional devolution rather than a conferment of sovereign rights as envisaged in the idea of a federation of the conventional type. The framers wanted a governmental system in which local initiative was blended with a strong central control" (p. 3). The author argues that the opposition parties had not only been critical, but they cooperated in maintaining and strengthening the Centre-State relations. He, however, suggests that the

Centre should make room for "the urge for autonomy" on the part of the States (p. 50).

The author seems, however, to have been more successful in mapping out than in tackling some of the important political problems in the Centre-State relations. Before 1967 the Centre-State relations could largely be regulated within the framework of the Congress Party; but Dr. Dube does not show to what extent the State Congresses operating within the context of State politics became autonomous centers of power; nor does he refer to extra-constitutional agitations that developed out of political controversies such as those over linguistic reorganization of States, the position of Hindi as the national language, and allocation of resources—controversies which could not be solved within the framework of the Congress. The book not only lacks a proper background, it also lacks analysis in depth of any problem in the Centre-State relations since 1967; in fact, the book is superficial. Much has been written on this subject in India and a number of papers have appeared in scholarly journals including this *Review*, but the book does not make any reference to these. Nor does the author mention the political process leading to the Congress split in November, 1969, and in particular the Centre's helplessness to check the breakdown of law and order in West Bengal—a helplessness that arose partly because of Mrs. Gandhi's dependence on communist support in Parliament. The book is, therefore, very disappointing.

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Kurt Riezler Tagebuecher, Aufsaetze, Dokumente. Edited by Karl Dietrich Erdmann. (Goettingen: Vandenhoeck & Ruprecht, 1972. Pp. 766. DM 142.)

Until his death in 1955 Kurt Riezler, professor at the New School for Social Research, was known to Americans mainly as a classical scholar. Generally unknown, however, were Riezler's political activities in the First World War, during which he produced one of the most significant political documents about Imperial Germany ever published.

In 1906 Riezler became a press adviser in the German Foreign Office. His performance attracted the attention both of Kaiser Wilhelm II, who expressed satisfaction "that such a clever man was won for the press office," and of Chancellor Bethmann Hollweg. From 1912 until July, 1917, as Bethmann Hollweg's closest confidant and aide, Riezler was at the center of German policy making and recorded his observations and impressions in notebooks and on loose papers. Later in life Riezler saw his early political career, in the words of his brother, "as a detour which distracted him from the actual center of his essence," and he ordered his notes to be destroyed after his death. But the efforts of such friends as West Germany's first president, Theodor Heuss, made publication possible.

On August 22, 1914, the Chancellor asked Riezler "for peace terms and [his] ideas" (p. 201, all quotes are my translation). At the time of the outbreak, Riezler had considered that the goal of German policy was to break up the Entente, not to fulfill dreams of territorial expansion. After the war began, however, his view changed. Riezler's famous "September Program," advocating limited annexations, a hard peace for France and a Belgian vassal state, was a temporary catalog of possible war aims. The Chancellor rejected the document but liked Riezler's idea of indirect domination, a sort of middle road between *status quo* and annexationism. Riezler observed that "Athens fell because of the brittleness of its constitution, because it could only conquer. Rome founded an empire because it understood how to expand its constitution and could incorporate the conquered" (p. 249). The outdated, brittle Prussian constitution should have been replaced by a "central European empire of the German nation."

This new empire would have been like a joint-stock company which could always absorb other firms: Riezler envisioned a "'federation of European states around the German Empire in which Germany has a majority of shares, just as Prussia has in the present German Empire" (p. 268). Riezler never denied that this and Bethmann Hollweg's "moderate power policy" had something in common with the war aims of the Pan-Germans and the military, namely the strengthening of the continental and global position of the German Empire. The new structure, however, would not have been an "offensive and seemingly domineering political structure" (p. 365). Riezler advocated copying England's practice of controlling without really seeming to control. Most important to Riezler, such a Europe, with Germany as its heart, could offer "an idea corresponding to the greatness of the time," which could serve as a barrier against the "West European freedom slogan. It would be terrible if this flat spirit should hold the world captive forever" (p. 210). Riezler never indicated exactly what such a great "idea" should be.

Riezler and Bethmann Hollweg agreed that the sturdiest foundation for this federation would have been the working masses and a supranational movement, not the "junker-industrialist leadership." However, Riezler became increasingly pessimistic that such a foundation could be laid: "All of Germany seems to have falled into two parts," he observed (p. 435). The split between left and right, which seemed to have been healed in August, 1914, became so aggravated by the strains of war that reform seemed no longer possible, and revolution seemed inevitable. "The wound, if it is not fatal already, will break open again and again" (p. 485).

Inner reforms and what Riezler and Bethmann Hollweg considered to be a "rational foreign policy" proved to be impossible because of Kaiser, military, and Prussian bureaucracy. Riezler especially despised the military, who had no appreciation for the difficulty of political business. He noted that "the concept of the purely military is celebrating orgies here" (p. 207). Germans' respect for the military placed political leaders in a weak position. "No one dares to speak against military necessity" (p. 217). The restraint required for "modern domination" could not be expected of Germany's ruling class, which "only believed in the steamroller." Bitterly Riezler wrote: "Never was a people more capable of conquering the world and more incapable of ruling it" (p. 461).

Revolution played an important role in Riezler's thoughts at this time, for he had a part in both the Bolshevik and German revolutions. Riezler and Bethmann Hollweg considered a separate peace with a weakened Russia much more important than a revolution, but Russia's determination to stay in the war led to a change of policy. The diary reveals nothing about Riezler's role in the fateful decision to transport Lenin in a sealed train from Switzerland to Russia, but it is certain that only German political interests motivated Riezler. By spring, 1918, Riezler, temporary head of the German diplomatic mission in Moscow, advocated open support of counter-revolutionary forces in overthrowing the Bolsheviks, who, in Riezler's opinion, could neither stay in power long nor be of any help to Germany; "Understanding? An entirely non-Bolshevik thought" (p. 465)! All was outdated, however, by Germany's military collapse and the German revolution, which Riezler, as the central government's representative in Bavaria, helped to crush. Riezler lamented: "The war ends in revolutions all around us" (p. 455). Riezler saw little hope for the future after the long destructive war. "Humanity will learn as much as nothing from all this misery; it will only wake up with a more confused head" (p. 435).

Riezler's diary is valuable to political scientists because in it he combines first-hand observation of methods of rule, military-political relations, European federation, and early German *Ostpolitik* with speculation on morality and ends in politics.

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Politics and Privilege in a Mexican City. By Richard R. Fagen and William S. Tuohy. (Stanford, California: Stanford University Press, 1972. Pp. viii, 209. $8.50.)

Utilizing the perspective of a single community (Jalapa, the state capital of Veracruz), Fagen and Tuohy have provided a fascinating, well-written account of politics in Mexico. Aware of the deficiencies of many earlier community studies within and outside the United States, the authors have focused their efforts not so much on the issue of who governs or who holds power as on identifying the sources of power and the impact of the nation's governmental system on the residents of a strategically located community. Jalapa is strategic in the sense that as a state capital in the core area of Mexico, it lies at the nexus between national and regional politics in a centralized, hierarchically structured polity. Acknowledging that there is little local autonomy in Mexico, the authors define the scope of their study as including any political activity, whatever its origin, that takes place within the community. Citing previous national and local studies, they embark on an analysis of Jalapan politics as an example of what they consider the norm to be at the subnational level in Mexico, that of a '"politically well-managed community" in which the forces of stability rather than those of conflict predominate.

Drawing on their own data and those of other studies outside the confines of Jalapa, Fagen and Tuohy establish that this is a politics characterized by centralized decision making, highly stratified patterns of participation, and considerable inequities in governmental responsiveness to demands and in the distribution of benefits to the citizenry. Because present inequalities are so great, and class-based political movements so weak, they seek to explain why there is extensive public support in Jalapa for the governmental system and acceptance of existing social conditions.

Their research findings suggest six reasons: First, access to the policy process is strictly controlled at all levels. Second, limited resources, low status, and restricted leadership opportunities in municipalities contribute to the exodus of local talent to state and federal levels. Third, the channels through which people can articulate their grievances are severely limited. Where public officials demonstrate a response to local issues it is a consequence of their concern with civil tranquility and these men's fears of losing their image as capable managers with hierarchical superiors. Fourth, the political culture shared by the haves as well as have-nots requires acceptance of the status quo as a necessary condition. Fifth, a random sample of political attitudes toward governance demonstrates that while there is near consensus among all groups on the uselessness of voting in local elections and the elitist character of decision making, most citizens feel that they can expect equal treatment by police and governmental officials. Sixth, Jalapa's informal system of governance is the consequence of "an accretion of past practices, now rationalized by bureaucratic methods, supported by those who are most advantaged, suffered by those who are least advantaged, and eased by a national economy that has been more successful than many in generating opportunities and—in their absence—hope" (pp. 130–31).

The outcome of this political system, the authors tell us, is one of dynamic stability, in which postrevolutionary governmental arrangements have shown a marked capability to absorb external or internal challenges to authority. This political order which maximizes the forces of stability and minimizes those of conflict they explain as the consequence of authoritarian practices tempered by flexibility and responsiveness to potentially disruptive issues, wide-scale public acceptance and legitimation of existing authority relations, depoliticization of the community, and generalized apathy.

One of the more interesting insights into Mexican politics to be gained from this book, and one that also has cross-national utility, is the authors' explanation of the prevailing deference toward authority and acceptance of the status quo: for them it is to be understood in terms of the continuance, regardless of the Revolution, of the tradition of corporatist thinking with its emphasis on "recognition and acceptance of one's station [as] the key to the good society" (p. 40). Later Fagen and Tuohy develop this point further by referring to the "still powerful tradition of Hispanic bureaucratic centralism" that pervades Mexican governmental organizations (p. 137).

In short, as a case study this book combines a precise methodology, conceptual clarity, and imaginative thinking in analyzing and evaluating the material collected.

Regardless of these merits, however, there is a problem of generalization which such a single, discrete study of the unique cannot resolve. I do not raise this point in the abstract, for if anyone writing about subnational politics in Mexico in particular and in Latin America at large has the skills, the resources, and the ability to engage in comparative analysis, it is Professor Fagen. Limiting this observation just to the studies for which he is responsible in one way or another and which the authors cite at random—Antonio Ugalde's analysis of Ensenada, Baja California (*Conflict and Consensus in a Mexican City* [University of New Mexico, 1970]) and Jorge Capriata's "The Political Culture of Marginal Elites: A Case Study of Regionalism in Mexican Politics," a comparative study of two cities in Chiapas—one is inclined to ask how do those cases compare with the findings reported in Jalapa? How do they differ? Can generalizations made here be converted into propositions regarding subnational politics in Mexico? If so, why? If not, why not? If the latter is the case then can one validly generalize about national-state-community linkages from a single instance? Unfortunately these questions are not considered in what is otherwise a fine case study.

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A History of the Chinese Communist Party, 1921–1949. By Jacques Guillermaz. Translated by Anne Destenay. (New York: Random House, 1972. [First published in France as *Histoire du parti communiste chinois*, 1921–1949, by Payot, Paris, 1968.] Pp. xviii, 477. $12.95.)

Although the Chinese Communist Party (CCP) came to power some 24 years ago, general and comprehensive accounts of the Party's eventful history from 1921 to 1949 are only beginning to appear in print in the West. Guillermaz's is one of the more recent arrivals.

Unlike earlier monographs, this study places the birth and growth of the Chinese Communist movement in broad historical context and yet never loses its focus on the CCP itself. The book devotes a good deal of space to such sociopolitical issues as the CCP's changing relations with the Kuomintang (KMT), the overall Chinese political scene during the Sino-Japanese war, the interaction between domestic politics and the international scene and the shifting world balance of power bearing on the outcome of the CCP's struggle for ascendancy.

The author is fully aware of the constraints placed on the study by the scarcity of reliable information because of the lack of free access to the archives in Peking and in Taipei, and the distorted nature of much of the available official and autobiographical materials. But this disadvantage, faced by all contemporary writers on CCP history, is in part offset by the author's personal experience in China, where he served as a French diplomat in the 1930s and 1940s. Guillermaz conveys a sense of reality not shared by many other armchair historians.

Although the author writes with a historian's pen, the material he presents is of value to political scientists who want a composite picture of what brought the Communist Party to power in China. His accounts of how the CCP administered the regions under its control, first during the Kiangsi period, then during the Sino-Japanese war, and later during the full-fledged civil war, present some coherent patterns anticipating the future administration of the People's Republic beyond 1949.

Guillermaz is quite right in stating that Mao Tse-tung's importance in the CCP during its initial years was not as great as the contemporary official history would have us believe (p. 57, n. 1). But his discussion of Mao during 1921–27 is too brief and scattered, except in one section (pp. 115–117), and no systematic account of Mao's role is given until almost the second half of the volume (after p. 242). Unless Mao's early emphasis on the peasantry and his disagreements with the Comintern-dominated CCP Central Committee are emphatically presented, the ordinary reader would not fully comprehend the reasons why the initial Comintern policy of urban proletarian uprisings in China failed so tragically. The author has glossed over the deep-rooted mistrust between Mao and Moscow dating from very early days. On the other hand, he has taken too literally Mao's 1949 statement about "leaning toward" the Soviet Union (p. 438) without examining the circumstances prompting that unusual statement. But Guillermaz shows good judgment in suggesting that Mao was pragmatic enough to adapt his views to changing times and circumstances, as in the scaling down of brutality used against the landlords and rich peasants during the Yenan period (p. 431).

Guillermaz's analyses of the basic issues are, with few exceptions, generally sound. One such exception is his argument that the awakening of the proletariat was a third factor—along with nationalism (May 4th Movement) and the messianic message of the Bolshevik Revolution—contributing to the origin and development of Chinese Marxism. He cites statistics to show the size and urban concentration of the Chinese proletariat around 1920, on the eve of the founding of the CCP. One can find grounds to quarrel with Guillermaz on this point: Not only was the 1.9 million Chinese proletariat three times smaller than the Russian proletariat during the comparable stage; but the Chinese peasant-worker ratio (with the peasant households amounting to 96 per cent of the total population, as Guillermaz noted on p. 9), in contrast to the 2:1 ratio in Russia, rendered meaningless the author's claim about the role of the Chinese proletariat.

Some minor errors or inaccuracies may be noted. Yeh Ch'ing (Jen Cho-hsuan) did not "disappear" as the author claims (p. 62), but joined the KMT after he had miraculously escaped death from the shots of a KMT firing squad in an execution of captured CCP leaders. He moved to Taiwan with Chiang's government in 1949, and became a leading anti-Marxist theoretician. The nominal head of the Party after the Tsun-yi Conference in 1935, which confirmed Mao's de facto leadership, was Chang Wen-t'ien, not Ch'in Pang-hsien as the author suggests (p. 257). Ch'in had been the Party's Chairman until then. General Marshall's mission to China should be described as mediation and conciliation, instead of "arbitration" and "reconciliation" as given in the book (p. 384). The date of the purge of Kao Kang, the Manchurian Party boss, is given as 1953 (p. 268), when in fact it was 1954. The Party leader who greeted Mao's arrival in Yenan was Liu Tzu-tan, not Liu Chih-tan, although the same mistake is made in several other English-language books.

Guillermaz gives the following as the principal "causes" of the Communist victory over the KMT (pp. 442–448 *passim*): (a) centralization of control; (b) better organization; (c) centrality of purpose provided by the Party's ideology; (d) the quality of the cadres; (e) the Party's missionary role; (f) the mistakes of the KMT, and (g) the Sino-Japanese war, which diverted the attention and sapped the strength of the KMT government and gave the CCP a respite and an opportunity to expand. This diagnosis is quite legitimate; my only question concerns his point regarding CCP's "centralization." Most scholars familiar with the CCP's and the KMT's working styles would agree that the CCP, especially under Mao's command, stressed *decentralized* operations and that Mao's strategy of decentralization enabled his guerrilla and regular army units to attain a high degree of mobility, with which the much more centralized KMT army was incapable of coping. Guillermaz could have meant "unity" within the CCP top leadership between 1936 and 1949, free of the factional strife that beset the KMT. If so, I would agree.

In summary, this is a handy, one-volume comprehensive history of the CCP from its inception to its rise to power, interlaced with many useful maps and photographs, some of which appear in book form for the first time.

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Urban Dynamics in Black Africa: An Interdisciplinary Approach. By William J. Hanna and Judith L. Hanna. (Chicago and New York: Aldine, Atherton, Inc., 1971. Pp. 390. $9.75.)

There have been two major approaches to social research in African urban areas. The traditional approach was concerned primarily to understand the mechanisms by which rural Africans adjusted to the imposition of colonial cities. Contemporary research, by contrast, tends to be less concerned with adjustment as such, focusing rather on the distribution of power, goods and services in the process of development. The implied purpose of much of the more recent research is to provide for a more equitable and efficient distribution of scarce resources over the whole urban community. The Hannas' book, a review and synthesis of the extensive secondary literature on African cities, emphasizes the first approach. It is divided into two sections: a chapter-by-chapter discussion of the main themes of the literature, and a long bibliography. As a general introduction to African urban conditions, this book should be welcomed both by Africanists and those interested in urbanization in the Third World.

Generalization about African cities is a risky business. Their great variety and number; descriptive material that is often superficial, outdated, and unsystematic; and the very great changes that have been taking place since independence—all serve to undermine even the most carefully qualified assessments. The Hannas treat this problem gingerly by suggesting that, although they intend to generalize, the statements they make must be viewed as a series of working hypotheses. Early chapters present material on the importance of African cities and their patterns of growth. The authors then move into the literature on rural-urban migration and the whole question of the degree to which African townsmen are "committed" to urban life. This leads logically to a discussion of social structure and, in particular, the importance of ethnicity as a situational variable influencing a wide range of urban choices.

While ethnic structures (neighborhoods, associations and friendship networks) represent an early phase in the adaptation of migrants to urban life, a later phase is characterized by the greater importance of what the Hannas call "quasi-classes." These they define as "influential population aggregates" which are at least "semiautonomous units." Examples of urban quasi-classes are the governing elite, modernists, businessmen, and wage-earners, each of which is described at some length. Unfortunately for the clarity of the argument, these quasi-classes are not mutually exclusive categories (is this why they are only *quasi* groupings?), nor is the political expression of their interests always free of "ethnic" considerations. The importance of these categories in urban politics is in any case downgraded by the authors themselves when they argue, in Chapter 8, that the recent development of class politics is still subordinate to or even submerged under ethnic politics. In fact, some of the best analysis in the book focuses on the different political forms taken by urban ethnicity and the different kinds of conflicts that result. Of particular importance is the situation in which a local ethnic group competes in various political arenas with a "stranger" group for control of a town. Almost invariably the "stranger" group is educationally and economically better off, though numerically inferior to the local group. In this form of competition class and ethnicity, sometimes even religion, merge and overlap so much that they are not easily separable by simplistic models of structural conflict.

While it is difficult enough to describe and analyze the social and economic basis of political conflict in African cities, it is an even more elusive exercise to grapple with the range of forms taken by African urban politics. This is particularly the case when the unit under discussion—the urban political system—is not clearly differentiated from either the national political system on the one hand, or the myriad of structures that link urban with rural areas on the other. To deal with these apparent ambiguities, the Hannas present a model of an urban political system containing three different subsystems—ethnic, locality, and interlevel—and subdivide each according to whether relations flow through a formal network, an informal network, or a political party network. The utility of this model for comparative purposes will have to be tested with more data than are presented in this book.

Two limitations of the study deserve comment. First, the authors rely too heavily on theoretical propositions from non-African literature, many of which are plausible, but have no demonstrated relevance to modern African cities. For example, an extended discussion in Chapter 9 of the role that "key townsmen" play in integrating the three urban subsystems makes important citations of, *inter alia, The American Voter,* a study by Eisenstadt in 1952 of the leaders of immigrant groups in Israel, an article by Lucian Pye written in 1958, Festinger's work on cognitive dissonance, and a general discussion on industrialization and society by Hoselitz and Moore. How key townsmen *might* function in modern Africa is one thing; how they in fact *do* function to integrate urban subsystems is a more interesting, but still unanswered question.

A second limitation is the book's relative silence on what we might call "the dynamics of urban policy." Although a useful chapter is devoted to urban problems such as unemployment, bad and expensive housing, educational deprivation, poor health and sanitation facilities, and "poverty pockets," no attempt is made to provide systemmatic explanations for their increasing incidence, or to discuss the range of possible solutions presently being explored by African governments. For example, to what extent can the present income structure be modified to reduce the elite/mass gap in African cities? How can the balance be redressed between bloated, often parasitic capital cities and stagnating rural areas? What kinds of housing policies would bring the benefits of urban planning to the lowest income groups? How can urban political systems be restructured so that a broader mass of citizens can participate in their own local governmental institutions? These are profoundly political questions in essence, although they are often technical in form. But their answers depend on an approach to urban development that both incorporates and goes beyond the concerns of this book.

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Turning Point in China: An Essay on the Cultural Revolution. By William Hinton. (New York: Monthly Review Press, 1972. Pp. 112. $5.95, cloth; $1.75, paper.)

Hinton's book is essentially a philosophic essay, rather than a political analysis of the Cultural Revolution in China (1965–1969). Hinton, one of the first American Quaker representatives in China, postulates that the Cultural Revolution in China "has not been a struggle over power for power's sake;" but rather "a struggle between individuals representing conflicting class interests" (pp. 16–17).

Examples cited by Hinton to illustrate his thesis include: (1) the development of "bourgeois elitism and profit motive" in China in the middle 'sixties; (2) the impact of "revisionism" in Soviet Union upon Chinese society; (3) the war in Indo-China; and (4) the "inability of the Chinese Communist Party [under Liu Shao-chi] to lead the country forward while split into two irreconcible factions" (p. 54).

Although he revised and completed the text while in China during the summer and fall of 1971, Hinton describes events in China in a way that is rather confusing and difficult to follow. A considerable part of his description seems badly dated and gives a misleading impression of how China is run.

Moreover, Hinton's oversimplification throughout the book seems to betray a certain political bias, e.g., "In China the working class is beating back the bourgeoisie. In the world revolution of today China leads the way" (p. 112).

In spite of many loopholes, the book is still worth reading. Hinton's essay on the Cultural Revolution in China does offer a third dimension of missing perspectives in our own analysis of Chinese politics.

WINBERG CHAI

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Modernization without Development: Thailand as an Asian Case Study. By Norman Jacobs. (New York: Praeger Publishers, 1971. Pp. 421. $20.00.)

This exasperating book portrays Thai society and seeks to explain the propensity of that society—and presumably others like it—for modernization and development.

According to Professor Jacobs, *Modernization,* is the "maximization of the potential of the society within the limits set by the goals and the fundamental structure (or forms) of the society." *Development* is something more—the maximization of social potential "regardless of any limits currently set by the goals or fundamental structure of the society" (p. 9). The author tells us further that ". . . qualitative change [is required] in all the crucial focuses of the social order, if development is to be achieved in any one crucial focus of that social order" (p. 12). Thus, it is possible to modernize without "developing," and Thailand is offered as an example.

Patrimonialism explains this lack of development. *Patrimonial* is defined in terms of the distinguishing properties of seven fundamental societal institutions—authority, economy, occupation, stratification, kinship and descent, religion, and societal arrangements for maintaining and adapting the social order (p. 13). Patrimonial authority is largely a function of personal status plus control over prebends that can go with such status. This authority sustains the moral quality of the system. Therefore the patrimonial economy can legitimately be manipulated by political authority. The essential occupations are farming and statecraft—but not in that order. Kinship and descent arrangements work against the accumulation of wealth; religion legitimates the social order and roles within it; and the statecraft-practicing moral-intellectual elite has a monopoly control over order and change. So goes the definition, or the model, as it is called by the author (p. 5).

The bulk of the book describes Thailand in terms of these seven fundamental "institutions" and exemplifies one of the methodological problems of social scientists: If you posit a comprehensive model, and apply it to a set of phenomena, then, by golly, the phenomena tend to fit the model—and appear to validate it. It is therefore not exactly surprising to find that Thailand is the epitome of patrimonialism.

Some of the most interesting material in this volume is found in three chapters (9, 10, and 11) dealing with religion. They contribute to the portrait of a monumentally coherent social system, distinguished by a superb capacity for persistence and expansive adaptation. Yet the fundamental normative properties of this social system thwart "development." Thai society fails to maximize its potential. It does not "manifest an open-ended commitment to productive change, no matter what the consequences might be on existing goals or existing ways of doing things" (p. 9), alas!

This book starts more arguments than it resolves. It is rife with sweeping generalizations and oversimplifications. For example, we learn that Thai peasants are neither economically passive nor indifferent. They hedge against danger; they resist unproven opportunities. In matters of credit and marketing they tend to be exploited. They depend upon informal organization to meet extrafamilial labor needs. But the Thai rural sphere is inordinately more complex than the author's generalizations indicate.

To sum up, there is the making of something interesting and useful here, but it is both overdone and underdone. The patrimonial perspective may indeed do double service as model and as explanation. But on the one hand it does not adequately handle certain salient phenomena—e.g., the "rural sphere" and the economic system. On the other, it doth profess too much—in offering a single, fundamental, coherent thematic explanation of the "modernizing" and "developmental" propensities of something as complex as Thai society.

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Revolution in Perspective: Essays on the Hungarian Soviet Republic. Edited by Andrew C. Janos and William B. Slottman. (Berkeley: University of California Press, 1971. Pp. 185. $10.00.)

The result of a symposium commemorating the 50th anniversary of the Bela Kun regime, *Revolution in Perspective* is a specialist's book conceived along relatively limited lines. The editors specifically make no "claim for completeness or for providing a comprehensive reevaluation of the regime" but rather seek to investigate several of the more controversial or less-illuminated aspects of the short-lived regime. While one might have preferred the inclusion of other topics (such as the response of Hungarian workers to the regime or, more important, the place and impact of the brief Soviet period on subsequent Hungarian history), one cannot help welcoming this addition—modest as it is—to the appallingly small stock of serious work on Hungarian communism.

By far the best written and most scrupulously documented as well as most persuasive essays of the study are Professor Janos's pieces—one on Hungarian politics and society prior to the Soviet Republic, and one on the agrarian question in 1918–19. Working from the hypothesis of the growing gap between the politics of development and the politics of mobilization in a modernizing society, Janos argues for a new look at turn of the century Hungarian society and particularly at the ruling elite in that period. Dismantling the traditional view of an *ancien regime* led by the landed aristocracy, Janos traces the creation and expansion of a new ruling elite, the gentry-dominated bureaucracy, which in time operates (and enters political alliances) according to its own vested interests (which "took precedence over the particular interests of economic classes" [p. 20]). Thus there are examples of significant splits between the landed aristocracy and the ruling bureaucracy, particularly on the issue of economic progress, as well as evidence of changes from traditional methods of rule (though not necessarily from traditional attitudes towards the ruled). Indeed, the eventual undoing of the new ruling class was the growing political awareness and expectations of the working class and peasantry, which eventually created insuperable economic as well as political dilemmas for the ruling gentry. The latter tended to alienate both the landed aristocracy and the growing bourgeoisie in efforts to solve these problems (primarily through economic reward and status rather than popular political participation). The gentry itself, however, became diluted or "corrupted" as it sought to absorb the ascending national minorities or potential middle class. It is not entirely clear why, as Janos maintains, the gentry was willing to sacrifice its own to accommodate talented German, Slovak, and Rumanian youth—on the demand of assimilation—while, according to Janos, it perceived the same absorption of already assimilated Jews as a threat to gentry rule of the bureaucracy.

In his second essay Professor Janos does not dispute but rather supplements the traditional view of Hungarian society, this time demonstrating that the well-known peasant opposition to Bela Kun's policies contained a vocal Left as well as the more generally emphasized Right. This "second agrarian opposition," composed of radical peasant socialists, maintained a large degree of independence, opposing the urban, industrial Socialists on numerous issues, condemning in particular the "new bureaucracy," dominated, they complained, by Jews.

Another essay which seeks to counter many of the accepted views on the competing forces within the new regime is that of Peter Kenez, who unfortunately does not present his case as convincingly as Janos does. While his point may be well taken, i.e., that the divisions within the ruling Hungarian Socialist-Communist Workers Party occurred along issue cleavages rather than along the lines of former party affiliation (Socialist or Communist), he oversimplifies when he states that there was no difference between Socialists and Communists as such in the postmerger period. He could perhaps more convincingly prove the final unity of the two groups on the key issues of agrarian and foreign policies, but he ignores the differences in attitudes and disputes which went on in practically every "ministry," albeit often because of the views of the Left Communists but no less real because of this.

Professor Keith Hitchins expertly analyzes the perhaps tragic dilemma of the Rumanian Socialists of Hungary—a dilemma similar to all the minorities of the country—and the failure of the Russian-trained universalist Kun regime to provide a satisfactory nationalities policy. Given, however, the editors' stated intention of providing two essays on "external" aspects of the regime, the discussion of Rumania itself and the Socialists within Rumania should have been treated in a separate essay. In view of the critical role played by Rumania in the destruction of the Kun regime, such an essay would have helped place events in their international context. Professor Slottman's entertaining and authoritative essay on the reaction of the Austrian Geistesaristokraten to the Hungarian Revolution of 1919, on the other hand, would seem irrelevant. Whatever the justification for examining the reaction of this group rather than that of leftist circles or the ruling circles in Austria, the result would tend to bear out the author's own suggestion that "perhaps such an inquiry is too blatantly the product of a curiosity that should be restrained" (p. 149). For all that Professor Slottman's fine scholarship can uncover is three people (Stolper, Musil, Kraus) who have anything in some way important to say about the events. Indeed much of this essay seeks to explain the "non-response," which itself might have been of value if it had had any real significance for Austrian society at the time.

The book concludes with an all too brief essay by Richard Loewenthal analyzing the place of the Hungarian Soviet in International Communism. Loewenthal points out that the international movement learned the wrong lessons from Kun's failure, preferring to trace the cause to the lack of a Leninist party rather than to specific errors of the Kun regime or to the basic fact that "the conditions for a purely Communist revolution had not existed in Hungary" (p. 178). While one might argue Loewenthal's contention that before 1919 Lenin did not consider his conception of a party as a model for others (what of the disputes with Rosa Luxemburg or Lenin's *Imperialism: The Highest Stage of Capitalism?*), Loewenthal accurately points to the impact of the Kun failure on the future policy of the Comintern in its emphasis upon organization and Leninist parties.

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Run it Down the Flagpole: Britain in the Sixties. By Bernard Levin. (New York: Atheneaum, 1971. Pp. 451. $8.95.)

Among the thousand and one developments that Bernard Levin discerns in Britain in the 'sixties is the decline of the written word "almost as if newspapers' managements and editors were determined to prove Marshall McLuhan right: by the end of the decade the number of journalists whose chief drawing power lay in their prose style had fallen catastrophically" (pp. 342–343). In that case Levin himself is, to use a favorite geological simile, "one of those granite survivals from the Jurassic Age which are occasionally to be found sticking up out of the more recent deposits" (p. 302) in the Holocene press of modern London. For his chief drawing power undoubtedly is his mastery of the written language. Clause upon clause, interwoven with subclauses, bracketed asides, and strings of well-chosen adjectives are used to construct sentences of great length and complexity but great charm and easy flow. Irony, wit, and the imaginative combination of the apparently dissimilar lace the mixture. Readers of Levin's columns in the London *Times* (he has quit the *Daily Mail* since the publisher's blurb for this book was written, to the enrichment of the Top People of Britain) should need no further recommendation than his authorship; for the rest let me state that, of all the books I have reviewed, this is the one which I probably enjoyed most reading.

What does it offer the political scientist? One might first say what it does not provide. It does not constitute a reliable, balanced record of the decade. There are odd slips of fact—thus Lord Robens is referred to as chairman of the National Coal Board since 1962 (p. 148): he actually took up his appointment on 31 January 1961. Rather more, there is the bending of fact to suit the twist of Levin's argument or the flow of his story. Thus, Mr. Wedgwood-Benn's two by-elections in Bristol South-East (referred to as Bristol South-West) took place on 4 May 1961 and 20 August 1963; but the tale of Benn's campaign runs better with the latter described as the second by-election in the constituency "in just over a year" (p. 183). Or, in a later by-election, the needs of the drama of the folk heroine of the Northern Irish civil rights movement, Bernadette Devlin, dictate the odds over which she triumped: a "constituency which had previously returned Unionist members with comfortable majorities" (p. 263). In fact, this area has had a Republican majority ever since partition, and Unionist members had only been elected when the Republican vote was split or when part of it abstained because the Republican candidate was too extreme. But it spoils the story to point out that a Joan of Arc was bound to win anyway.

As for balance, Bernard Levin has his own idiosyncratic gallery of rogues. Prominent in it are all men of the law; the pompous hypocrisy which often affects judges and counsel is unmercifully sought out and laid bare. The unfortunate Mr. Henry Brooke, British Home Secretary for two years in the early 'sixties—who had the ill-luck to hold an unpopular office when the satire boom got under way in Britain but whose chief offence is less what he did than being so bad at explaining why he had done what he had done—is flayed alive.

Yet in his treatment of Henry Brooke, as in many other instances, Levin is fulfilling a valuable role: that of portraying the feel of the time, the indefinable mood of the moment. Henry Brooke did become, while holding office, a symbolic hate figure. *Run it Down the Flagpole* captures many such passing intangibles and, for one who lived through the decade in Britain, brings back many memories with a flavor of actuality. Not only of the changing mood of politics: Levin ranges from the burning of cattle in the great foot-and-mouth epidemic (likened to an ancient ritual burnt offering) to the Moors Murders (which includes a characteristic interpolation expressing Levin's dislike of Maoist China), with considerable attention to the changing morals of the decade and the more ridiculous attempts of the Law to regulate that process. An unforgettable chapter gives his appreciation of what he regards as the three great experiences of London operagoers. This volume is a masterly parade of insight, appreciation, experience, and prejudice covering the range of man's activities in Britain during the decade.

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Village and Bureaucracy in Southern Sung China. By Brian E. McKnight. (Chicago: The University of Chicago Press, 1972. Pp. xi, 219. $9.00.)

In Western sinological scholarship the politics and institutions of the Southern Sung period (1127–1279 A.D.) have until very recent years come in for little attention compared to many earlier studies of the Northern Sung (960–1126). Professor McKnight's thorough and lucid monograph treating a highly complex subject very creditably reflects this burgeoning of scholarly endeavor on the Southern Sung. The author also blazes a relatively unexplored trail by penetrating to the most elemental strata of Sung governmental organization that historical resources permit—the village-level subbureaucracy composed of clerks and village officers.

It is the village officers conscripted on a rotating basis from the rural elite, defined as the top four tax classifications of households, that are the focus of this study. While in later dynasties the power and functions of rural control were distributed among several groups, during the Southern Sung, village officers (and in particular the very crucial Superior Guard Leader) were alone responsible for tax collection, the maintenance of local order, the compilation of land and population registers, and other miscellaneous services. Indeed, these village officers, both wielding and yielding to political authority, were the lynchpins in the government's control of society.

Although the author claims only to make a descriptive analysis of the village service system —a goal which he admirably fulfills—he reaches conclusions and suggests generalizations about the Sung state and society that will interest readers of this journal. By way of technically describing the various procedures and criteria for the selection of village service personnel, the central chapters of the book delve into the administrative structure of rural society. The author emphasizes that government's conscious elimination of the rural elite's privileges, allowing them to be exempt from or irregularly to dodge village services and other obligations, made the Southern Sung rural society more equal and homogeneous, less rigidly hierarchical than previously. Wealth became the primary criterion for determining both the distribution of service responsibilities and local influence, so society became more open and fluid, just as the path into officialdom began to widen in the Northern Sung. The long-range potential effects of this change in the rural social order, largely brought about by the growth of a money economy, were completely disrupted by the Mongol takeover, however. Chapter 8, the penultimate chapter of the volume, examines several solutions to the most vexing problem of the village service system—the distribution of its financial burden among the people—and offers insights into the process of administrative innovation in traditional China. Initiation for reform, we are told, originated with the rural elite themselves who, with subsequent government encouragement, established in some regions public estates from which revenue was derived to pay those performing village services.

One minor criticism of this book: There are several typographical and Japanese romanization errors in the bibliography. Finally, without intending to diminish the considerable value of this work, I would hope that now after constructing the institutional and historical framework of rural society, the author will in the future permit himself to explore Sung society in more theoretical and even speculative terms and to relate his keen preliminary observations on the topic more fully to economic developments.

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Russian Peasant Organization before Collectivization: A Study of Commune and Gathering 1925–1930. By D. J. Male. (Cambridge: Cambridge University Press, 1971. Pp. 253. $12.50.)

American specialists on the Soviet Union owe a considerable debt of gratitude to their British colleagues for a noteworthy series of systematic studies of the Soviet Union in the 'twenties. This group of scholars, many of whom are associated with the University of Birmingham, have undertaken the task of filling in the gaps in our knowledge of this vital period, when so many of the basic decisions which ultimately shaped the Soviet system were being formulated. The book under review is an important contribution to this series. It focuses on the traditional peasant commune (*mir*, or *obshchina*), which remained the basic institution of peasant land management in most of the Russian-inhabited territories of the USSR before collectivization.

The author's main thesis is that the structural and behavioral patterns of the commune, Western speculation notwithstanding, in no way facilitated the introduction of the collective farm. Indeed, the commune, as a more or less natural peasant institution—as evidenced by its vigorous spontaneous revival after the Revolution—represented a serious obstacle to Bolshevik efforts to penetrate the village. When the collectivization drive began, the agents of the regime found it necessary to circumvent the commune or to smash it, since they could not use it for their purposes. Male suggests that very few Bolshevik leaders had any illusions about the commune. Most of them deplored its technological backwardness and its tendencies to preserve traditional social patterns. A few, following Bukharin's Stolypin-like injunction to "wager on the strong," looked with favor on the establishment of independent farmsteads. But the majority found this ideologically unpalatable. In any case, for most of the period, Party leaders recognized their inability to influence village developments. Legislative efforts to increase the power of the rural soviets vis-à-vis the communes were stillborn until the eve of collectivization, as were efforts to enforce improvements in cultivation practices. The harder the regime tried to break the power of traditional village authorities, the more the peasants banded together in their communes. Party influence in the village was minimal. The position of the local soviets was not much better. Dependent on the peasants for much of their financing, the soviets often became tributaries of the communes they were supposed to control.

The author's principal contribution consists in (1) a painstaking comparative analysis of physical characteristics of communes in different regions of the RSFSR—drawn mainly from statistical handbooks and published secondary accounts of the period; (2) an exegesis of basic legislation on land tenure and the formal powers of various local political institutions; (3) a comparative study of budget data of local soviets and communes. The book contains more than fifty tables, several useful organization charts, four maps, three appendices, and a moderately extensive bibliography. Specialists will find much here that is of interest.

Yet the presentation of the materials, the very wealth of detail on certain questions, is sometimes rather disconcerting. The data are not always effectively marshalled in support of the basic themes. One has the impression at times that the argument is merely a vehicle for the author to present his hard-earned data. This impression is reinforced by Male's frequent disclaimers that the data available don't really tell us what we need to know or seem to be seeing. Thus, his very detailed treatment of regional physical variations among communes is accompanied by frequent caveats against hasty generalizations about what the commune in fact was as an institution (p. 53). This is rather a modest reward for wading through so much material.

Similarly, the value of the detailed discussions of legislation is somewhat reduced by Male's admission that most of the important provisions remained a dead letter during the period (p. 109). Again, the actual importance of the matter under discussion does not seem to warrant such thorough treatment.

At the same time, it seems to me that Male needlessly restricts the scope of his study. He purposely chooses to deal with some important issues in a cursory way. Thus, he passes over the questions of the penetration of machinery and modern techniques into the village and the development of a market orientation much too facilely. While this is not true of his treatment of social stratification, he tends to rely rather uncritically on Soviet assessments of intravillage relationships. An analysis of family size as a factor in household socioeconomic status would have helped to clarify some of his own questions concerning the standard Soviet assessments.

There are also a few minor points of interpretation that may disturb the reader. It is certainly at least misleading to assert, for example, that Lenin "did not oppose the Stolypin reform" (p. 158), although it is true, as Male points out, that he had little use for the commune. And there are by now many Western specialists who would quarrel with Male's assumption that "collectivization was a decision made . . . mainly on economic grounds" (p. 212), unless he tells us what he means by "economic."

Naturally, there is little use in criticizing an author for not writing the book the reviewer thinks he should have written. Male is certainly to be praised for the high standards he observes in the interpretation of his data. We should be grateful for the wealth of material he has provided and the insights he brings to the complex problems of the Russian village on the eve of collectivization.

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Sous-Développement et Révolution en Amérique Latine. By Ruy Mauro Marini. (Paris: François Maspero, 1972. Pp. 199. F 14.80.)

Marini's book is mistitled. It deals almost exclusively with Brazil, more specifically with the events culminating in the 1964 coup d'état and its aftermath to 1969. Only in the short introduction and conclusion is there an attempt to link these events with those occurring elsewhere in Latin America. These "underdeveloped" countries are seen as pawns at the mercy of international market manipulations over which they have no control. Since these manipulations are reinforced by the actions of the local bourgeoisie, who in turn benefit from them, the proletariat finds a double barrier to achieving its just rights: In the local arena the bourgeoisie is content to keep in power governments that maintain or even expand its privileged status, while, for a price, nationalism is also sacrificed to the highest international bidder.

Marini is personally acquainted with and closely linked with the Revolutionary Left in Brazil. But such familiarity is insufficient, for the author engages in vast simplifications and his analysis does not go much beyond a chronology of events. There is little grasp of the Brazilian and Latin American background; moreover, the analysis is tainted with Marxist clichés that do little to explain the Brazilian situation and even less to place it in the broader framework of Latin American development. As an example, the author affirms that "the most original Latin American contribution to the world proletarian struggle [is] its international character" (p. 27). This said, he goes on for two long chapters (most of the book) examining capitalism and revolution in Brazil, without demonstrating if/how the Brazilian proletariat may function as an actor in the liberation struggle that presumably other proletariats are waging elsewhere.

Similarly, Marini is all too easily convinced of the revolutionary spirit of peasant leagues, urban syndicates, and even military elements. He fails to explain Che Guevara's difficulties in finding much support for his ideology in the Bolivian *campo*—difficulties that probably would be just as pronounced, if not more so, in the Brazilian interior. Further, ideological blinders prevent Marini from considering the fact that in Brazil, though Marxist ideology did have the support of a few elements in society, the great majority among the less privileged groups seem to have been moved more by personal motives, by nationalistic considerations, by populist-paternalist appeals, and by the lack of clear leadership from the soon-to-be-deposed President Goulart. In addition, the author fails to account for the role of the middle class in aspiring to and imitating upper-class values rather than in serving as a vehicle of revolutionary fervor to lead the lesser privileged elements of society.

In the concluding chapter Marini is optimistic in predicting that the Revolutionary Left will eventually succeed in Brazil and in the rest of Latin America. But again this optimism is not buttressed by facts; the author's ideological faith appears to enable him to dismiss fact and to forget historical and sociological analysis. The final paragraphs of the books are another testimony to this type of interpretation. Here the author envisages the heirs of Che Guevara as leading and winning the struggle to "expropriate the expropriators," just as they had been doing "in Bolivia, in Chile, . . . and Cuba" (p. 188). By the time the book had been translated and published in France, Bolivia itself was again under a new rightist regime, and neither Chile nor Cuba are quite the models of success that Marini envisages.

Marini's interpretation thus both gains as well as suffers from his personal commitment to the revolutionary cause. It gains by giving the reader an intimate and personal account of the role of the Revolutionary Left in Brazil. By the same token, the personal perspective has prevented the author from transcending his own biases, from extending and deepening his analysis, and especially from dealing with issues posed by other interpretations of the same events. Finally, by explicitly accepting Cuba as the sole model to be followed, Marini fails to explore the possibility of viewing contemporary Brazil in its *own* terms and as another alternative model of economic development. These ideological biases and failures thus limit the value of this book for the serious student of Latin American and, most specifically, of contemporary Brazilian development.

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Struktura i funkcje partii chlopskiej na przykladzie ZSL (Structure and Functions of a Peasant Party: A Case Study of the Polish United Peasants Party). By Zenon Mikolajczyk and Emil Patryn. (Warsaw: Ludowa spoldzielnia wydawnicza, 1968. Pp. 331. 18 zlotys [approx. $.85], paper.)

System Partyjny PRL (Poland's Political Party System). By Michal Sadowski. (Warsaw: Ksiazka i Wiedza, 1971. Pp. 280. 22 zlotys [approx. $1.00], paper.)

Studies in the Polish Political System. Edited and translated by Jerzy J. Wiatr. (Wroclaw: The Polish Academy of Sciences' Press, 1967. Pp. 242. 35 zlotys [approx. $1.60], paper.)

Eastern Europe's gradual post–World War II recovery has been accompanied by the re-emergence of the powerful scholarly traditions in the social sciences which prevailed in a number of these societies prior to Communist rule. In Poland, this trend has meant a new interest in the work and methods employed by prewar sociologists such as Czarnowski, Chalasinski, Grabski, and Znaniecki. Notable empirical efforts, even in the new and sensitive field of political sociology, have followed in recent years. In this latter area, despite significant political and ideological restraints on research, much of the work done by Poles shows an awareness of and interest in methods and substantive issues taken up in the West. As a result there exists a real possibility of communication and comparative research between American and Polish social scientists.

The Wiatr-edited work, the only one of the three reviewed that appears in English, is obviously of most direct use to American political scientists. This book, composed of ten articles which appear to have been published earlier in Polish journals, confronts a number of subjects central to American empirical and theoretical research. Divided into three sections, the articles cover aspects of "social change and social structure," "political parties and interest groups," and "local politics." Of interest in Part One are Ostrowski and Przeworski's factor analysis of mobilization and modernization processes affecting the Polish countryside, and Wesolowski's detailed discussion of diminishing class differences in postwar Poland. The second section of the book is mainly interesting for its description of Poland's curious electoral system. Also included is a Wiatr article which tries to define Poland's "hegemonic" party system (the dominant Communist Party along with two minor, but "co-ruling," non-competitive parties) in terms of a broad typology of comparative party systems. In the last section, Herz and Jasinska take up the question of interest-group conflict in local affairs, and another Jasinska article analyzes differing political roles adopted by local officials.

A major weakness in the book, probably the result of shortening longer Polish pieces in this translation, is the frequently inadequate descriptions of method and data—this is especially true of the Ostrowski-Przeworski and Jasinska articles. Nevertheless, as a whole, the book offers much to those interested in broadening their courses in comparative political systems by including material on a Communist state.

Mikolajczyk and Patryn's excellent effort is a case study of the program, organization, membership, and functions of the United Peasants Party, one (along with the much smaller Democratic Party) of the two minor political formations which continue to exist in present-day Poland. The UPP is a party with 400,000 members, compared to the Communist Polish United Workers Party's 2,100,000. An immense amount of well-organized information about this party is presented by the authors, who give the reader a picture of internal peasant party life unavailable until recently for the PUWP. Based to a large extent on surveys as well as statistical data, the book investigates why people join the UPP, how members differ from nonmember peasants, and what role is actually played by the UPP in Polish political affairs.

In contrast to Mikolajczyk and Patryn's modest case study is Sadowski's work, a revised and expanded second edition of his *Przemiany spoleczne a system partyjny PRL* (*Social Changes and Poland's Party System,* 1969). This book, clearly written in the tense political atmosphere of the last Gomulka years, is in reality several rather unconnected works in one. Probably of least interest is the author's lengthy defense of the Communist Party's "leading role" in the three-party system and his harsh attacks on the work of Wiatr and the émigré sociologist Zygmunt Bauman for their inadequate comparative typologies of political party systems. Sadowski also criticizes those sociologists concerned with interest groups. Of much greater value is the author's effort to compare the social bases, occupational structures, and educational levels of the memberships of the three Polish parties over time. Basing his conclusions on these interesting data, Sadowski finds that the Communist Party must be understood to be the single legitimate national political representative, since it completely monopolizes politically involved persons seen to be leaders in society, i.e., the educated, the professionally trained, the urban, the young. In this analysis, the two minor parties are characterized as mere representatives of specific "friendly interests"—the farmers and privately employed craftsmen—which have no possible pretensions to national leadership. That such a view coincides with long-time Communist Party policy cannot however erase the fact that in this work, for the first time, detailed comparative data on the Polish parties are presented in a conceptually interesting fashion by a Polish scholar.

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Dilemmas of Growth in Prewar Japan. Edited by James W. Morley. (Princeton: Princeton University Press, 1972. Pp. 527. $12.50.)

"What went wrong?" with the Japanese polity in the two decades before Pearl Harbor:

this question set the tone for the first American efforts to understand modern Japanese history and now returns for re-examination in this sixth and last volume of the Studies in the Modernization of Japan series.

It is instructive that this volume was not originally planned when the Conference on Modern Japan was launched in 1959. The five volumes then envisioned divided modern Japan into disciplines—history, economics, sociology, political science, and arts and letters—under an overall focus on Japan's "success" at modernization. This was a natural emphasis for the generation of scholars who were in most cases born of the wartime need for Japan specialists and had an active role in the Occupation reforms. By 1958, when this project was conceived, it seemed that Japan had indeed been reformed, and the 1930s receded in importance as a period in which Japan was victimized by a flawed (and ultimately reformable) political system. Recent research on the Meiji Restoration had encouraged a high evaluation of nineteenth century Japan, and the natural tendency was to judge Japan's modernization on a secular scale as a tale of success.

But the disaster of the war was too large and too close to be reduced to an interlude, and the old nagging question of "What went wrong?" could not be avoided for long. So this sixth volume was added midway through the series, and contains thirteen papers presented at the seminar held in Puerto Rico in January 1968. The authors are predominantly historians and predominantly non-Japanese (nine out of fourteen in both cases), so that the overall tone shows no great departure from the bulk of existing scholarly literature in English on modern Japan. James Morley in his introduction provides a thorough and competent review of the historiography (both in Japanese and English) of Japanese militarism, proving if nothing else that historical interpretation of that era has from the start been deeply mired in contemporary politics, particularly on the Japanese side.

Papers by James Crowley and Akira Iriye deal with Japanese decision makers' distorted images of the rest of the world in the 1930s, in the former case vis-à-vis China and in the latter, America. One is left persuaded that the Japanese were indeed astonishingly parochial in their views, although one would wish explanation of why the Meiji Japanese seemed so much more cosmopolitan: a study of the educational system on this score would seem in order. One would also wish for a more generous elaboration on Iriye's suggestion that the Japanese images were distorted partly because they tried to make sense out of a world which was itself in crisis and made no sense. What, for example, of the revisionist argument that it was American rather than Japanese imperialism which paved the road to Pearl Harbor? This view is of course a political booby trap, being equally popular among the American New Left and the Japanese New Right, but it deserves more attention in this volume.

A similar sense of Japan's confusion in functioning within the contemporary world order (or disorder) is conveyed in Hugh Patrick's lucid analysis of "The Economic Muddle of the 1920's." Assuming but not treating systematically the causal relationship between the economic problems of the period 1930–32 and the rise of militarism, Patrick deals largely with the monetary and fiscal system, offering the suggestion that a single policy change might have made a great difference: the return to the gold standard should have been effected earlier (around 1922 rather than 1930) and at a devalued rate (rather than pre–World War I parity). Such a concrete assertion of how things might have gone right (or at least less wrong) is refreshing in this generally overcautious volume.

George Beckmann and Peter Duus deal with left-wing dissenters who perceived that something *was* going wrong but failed to get their message across—the Japanese Communist Party in one case and professor/politician Ōyama Ikuo in the other. One is left with a shrug of the shoulders: The left was too preoccupied with Western political dogma to understand the political realities in Japan, which swept by them without a passing glance. Of far greater interest is Tetsuo Najita's brilliant paper on the politician Nakano Seigō as a representative of an indigenous radical tradition springing from Ōyōmei Confucianism. While leaving unanswered the question of how widely this ideology pervaded the modern political elite, Najita's essay is highly provocative and should prove seminal in the history of modern Japanese political thought.

Only two of the papers deal with the curiously neglected problem of what the conventional men of political and economic power in Japan were doing in the 1930s. Robert Spaulding's essay on the bureaucracy and Arthur Tiedemann's on big business serve to scratch the surface but leave one tantalized for more extended and systematic treatment.

Finally, two provocative papers treat the broad social forces underlying the political developments of the 1930s. Hayashi Kentarō effectively compares the modern political development of Japan and Germany in what should serve as a model for future comparative efforts. By avoiding an approach which begins with the sticky problem of defining "fascism," Hayashi comes up with lucid new insights into the nature of Japanese militarism in contrast to Nazism. In particular, he suggests that Japanese militarism represented a reassertion of traditionalist conservatism over the brief threat of the radical right, whereas in Germany Hitler was enabled to *use* the strength of a similar radical rightwing movement to come to power. A rather similar conclusion is reached by Ronald Dore and Ōuchi Tsutomu in their analysis of the role of the peasantry in "what went wrong." Following a cocky but persuasive rebuttal, line by line, of Barrington Moore's arguments on the Japanese case in *Social Origins of Dictatorship and Democracy*, the authors offer an alternative thesis: The Japanese village neither caused nor supported "fascism," it simply offered no resistance to it, being a stronghold of the "collectivist ethic." This still leaves unanswered the question of which classes *did* support fascism, and leaves open the possibility of a modification of Moore's thesis, namely, that enlightened agrarian reform might have made of the peasantry an active force *against* militarism.

The concluding essay by Edwin Reischauer, "What Went Wrong?," makes a dogged effort to pull together some of the interpretive threads presented in this volume, but in his effort to be fair to the diversity of suggestions made, he leaves the entire problem murkier than ever. Read in conjunction with Morley's introduction, this conclusion would suggest that we have moved from consistent if simplistic interpretations to such a tolerance of complexity that we end up with no interpretation at all, merely a list of "factors." Synthesis remains for the future, but many new avenues of approach are provided by these essays.

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The Modernization Imperative and Indian Planning. By Baldev Raj Nayar. (Delhi: Vikas, 1972. Pp. 246. Rs. 30.)

Professor Nayar of McGill University calls this book a product of interdisciplinary research, and sets out to examine the political economy of Indian planning. He attempts to analyze the political factors behind Indian economic policies by tracing the origins and political motivations of the Mahalanobis model, which formed the basis of the Second Five-Year Plan and Indian development strategy in the 'fifties.

The first chapter argues that in developing countries it is imperative for the governments to formulate programs and policies with the objective of accelerating economic development. Without such efforts, he suggests, any government would find it very difficult to survive. The success of such development programs would depend upon the type of political framework that exists in any particular country. Compatibility of economic policies and the political environment in which they operate, are analyzed in terms of a taxonomic model which distinguishes between the mobilization, the reconciliation, the bureaucratic, and the theocratic systems.

In the next chapter, Nayar tries to test his "model" by analyzing the compatibility of the Indian political system and the economic policies of the government. He begins with a fairly conventional discourse on the theories of balanced and unbalanced growth. It is shown that the First Plan, with its limited aims of providing the economy with a basic infrastructure, was quite consistent with the conciliatory political system in India. The Second Plan, with its emphasis on heavy industry and long-term growth, was more ambitious. It is argued that the Mahalanobis model was akin to the Soviet model. Its so-called "socialist" objectives were reinforced by the views of the political elite and the foreign economists (e.g., Lange, Bettelheim) who advised the planners during that period.

The third chapter contains the crux of the author's thesis. Economists generally specify growth and raising the level of consumption per head as the objectives of development. Improving the level of living is the basic motivation behind development programs formulated in developing countries. The author dissents from this view. He asserts that plans for economic development are rooted in the aims of political sovereignty, military security, and economic independence. Japan, China, and the USSR are cited as examples of countries in which a major motive force behind development programs was the quest for national security. Military strength and political and economic independence are seen as interdependent. The political and economic factors underlying the basic philosophy of the Second Plan are treated as a case in point. To substantiate his argument, Nayar quotes extensively from Mahalanobis and Nehru. Through their writings and speeches it is shown that industrialization is imperative for economic and political independence and national strength (pp. 109–134). Many of Nehru's speeches and ideas on planning go back to the pre-independence period. From this he concludes that the strategy of heavy industrialization of the Second Plan was rooted in much earlier nationalist political thinking. It was then that the imperative of modernization was born.

The last chapter reviews India's economic performance during 1950–1967, and finds that it compares favorably with the economic stagnation in the first half of the century but not so well with the contemporary growth in other developing countries. The strategy adopted in the Second and Third Plans did provide the country with a substantial infrastructure and industrial base, but at a cost. Most of the expansion took place in the private sector, contrary to the "socialistic pattern." The neglect of agriculture led to a serious crisis in the mid-'sixties. This was accompanied by inflation and unproductive defense expenditure. In terms of social welfare, we are told, matters became much worse, with increasing unemployment and widening inequalities. The dependence on foreign aid did nothing to improve the situation. In contrast to China's growth, India's economic performance appears relatively poor, but this is ascribed to the difference in political systems. (Recent evidence does not bear out the thesis that Indian growth has been less than Chinese.)

It is difficult to accept the author's main thesis. To say that the quest for military security and national political and economic power is *one* of the motivating forces behind development efforts is plausible; but to say that it is *the* raison d'être is gross exaggeration. It is curious to note how often political scientists, setting out with the intention to treat a subject in an interdisciplinary manner and to regard society as a *system,* end up asserting the dominance of politics. In most less developed countries, particularly during the latter half of the twentieth century, raising the standards of living of the mass of the people at subsistence level, is an important propellant in development programs. Indeed, as Nayar himself argues at the beginning of the book, the survival of governments depends upon such an effort.

In his attempt to prove the point, he has drawn on quotations from Nehru's speeches before and immediately after independence. Clearly, it is not difficult to find the desire for political independence and national power in these—it was a part of the nationalist movement. Attempting to reach self-sustaining growth through import-substituting industrialization was at least partially a reaction to the colonial past with its emphasis on plantation crops for export. It would have been interesting to examine to what extent the alleged trade-off between growth and equality was really a sacrifice of *both* economic growth *and* social equality for the military-industrial complex. But to identify the desire for economic independence with a quest for political and military power is to carry the argument to an extreme. The strategy of heavy industrialization was also prompted by economic factors. The slogan of self-sufficiency stemmed (at least partly) from the foreign exchange constraint on development and the export pessimism of the planners at that time. Economists may have overemphasized these factors, but one would have expected an interdisciplinary study to put them in perspective and to bring out the conflicts, not to dismiss them altogether.

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A Theory of Ideology: The Tanzanian Example. By John R. Nellis. (Nairobi, Kenya: Oxford University Press, 1972. Pp. xii, 217. Shs. 25/–.)

In *Opinions and Personality,* M. Brewster Smith and his colleagues asked, "Of what use to a man are his opinions?" Like other analysts of political ideas, John R. Nellis attempts to answer this question. As a political scientist, rather than a social psychologist, Nellis assesses the usefulness of ideology not only to an individual, President Julius Nyerere of Tanzania, but also to the Tanzanian political regime. Although explicitly rejecting both a functional and an individualistic social psychological perspective, he implicitly does adopt a functional framework. For him, the key dependent variable is system persistence, with the focus on the utility of ideology to the political elite and the governing structures, especially the Tanganyika African National Union and the planning mechanisms. In furthering the persistence of the regime, Nyerere's political beliefs serve to justify the present regime activity, to explain the meaning of events to the Tanzanian citizens, to promise future payoffs, to guide the process of decision making, and to rally mass support behind the elite and their development projects for the building of African socialism within Tanzania. According to Nellis, the ideology of Nyerere performs these functions by serving as a verbal currency, a standard of "deferred payment," in effect a functional substitute for money.

Of what use to a social scientist is this political economy model of ideology? In Nellis's view, a useful model of ideology, although necessarily oversimplified, should postulate assumptions that at least approximate empirical conditions. He wants his model to help explain political events within the independent African territories. His model leans heavily on classical economic assumptions originally devised by Adam Smith. In this ideal type, man is motivated by feelings of envy to seek greater income and status. The variables of supply and demand best explain the behavior of elites and masses. At least in the short run, the citizens' demands for concrete services cannot be met by the limited supply of resources, particularly in a poor country like Tanzania. The elite therefore offer symbolic promises of future payoff as a form of supply. Of course, in the long run, the gap between promise and performance will become accentuated, thus leading to frustration and eventual overthrow of the ruling personnel. Taking a pessimistic view of the prospects for political stability within the new African states, Nellis seems to deny the long-range utility of ideology for system persistence. Instead, he sees income production, organizational efficiency, and the manipulation of information as the most effective ways to maintain the leaders in office.

Of what use to a social scientist is this book? As the title implies, the book consists of two parts: a long articulation of the functions of ideology and an application of the model to the Tanzanian case. The first part contains more strengths than the second. Nellis sketches an original, imaginative account of the functional importance of ideology and its effects on public policies. He skillfully states his major assumptions, clearly justifies their meaning, outlines the deductive implications, and then criticizes their empirical validity within Africa. But the attempt to apply these assumptions to the Tanzanian situation meets with less success. Any ideology, including African socialism, has meaning only within a particular historical context. The same set of ideas has different consequences under diverse cultural and structural conditions. Nellis introduces little information about the precolonial cultural values or structural patterns of diverse Tanzanian groups. He also gives only limited attention to the postindependence power structures. Basing his evidence on fieldwork done in 1965–1966, he relates ideology to policies and policy consequences—especially to village settlement agricultural projects and to the Arusha plan—but not to the process of decision making. We cannot really estimate the effectiveness of ideology as a guide to decision making, because the description of economic planning remains separate from the treatment of ideology. Furthermore, in the absence of depth interviewing or survey studies, it is difficult to assess the meaning of Nyerere's ideas to the peasants and political elites as well as to ascertain precisely the extent to which his ideology explains the political meaning of events or rallies support behind the regime. Finally, despite the perceptive self-criticisms of his model presented at the end of the book, some of Nellis's assumptions derived from classical English economists may run counter to the African context. The skeptical reader will wonder about the applicability of political economic notions originally devised in an emerging industrial, private-capitalist society like England to explain conditions within agrarian, noncapitalist African territories.

In sum, this book, one to provoke thought about the importance of ideology in the developing nations, probably has greater utility as an explanatory model in general than as an aid to explaining specific political performance in Tanzania.

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Cuba: The Measure of a Revolution. By Lowry Nelson. (Minneapolis: University of Minnesota Press, 1972. Pp. 242. $10.00.)

In this critical study of the Cuban Revolution, Nelson attempts a systematic appraisal of the Castro regime's performance over a twelve-year period by comparing revolutionary Cuba with the Cuba of old. He has done so largely on the basis of available statistical data on economic production, national income, employment, education, public health, and other similar indicators of development for the pre- and post-1959 periods, and on the basis of the qualitative aspects of economic, social, and political change since 1959. The principal strength of this study for the Cuban specialist in particular thus lies in the compilation of often highly revealing data, and in the comparisons drawn with respect to two distinct periods in Cuba's contemporary development.

As a rural sociologist who studied Cuban society first-hand in 1946, Nelson is able to illuminate many social and economic aspects of prerevolutionary Cuba later distorted by both the Castro regime and its detractors alike. In early chapters, therefore, he examines the island's levels of economic and societal development before the Revolution, and the state of Cuban agriculture prior to 1959. The picture that clearly emerges—both descriptively and statistically—is of a society that was making advances on several fronts. Hence, Cuba showed encouraging signs in terms of modest industrialization, greater agricultural production and diversification, rising per capita income, and broader educational, health, and welfare benefits.

The balance of the book is devoted to a critical examination of the Castro regime's record in promoting economic development, social change, and political restructuring, with statistical evidence being employed where possible for comparisons with prerevolutionary Cuba. Nelson concedes that the Castro government has shown much greater integrity than its notoriously corrupt predecessors, and that it has produced some notable social achievements—especially in promoting public education and reducing prostitution, and possibly in promoting public health and some forms of cultural change. Nevertheless, he maintains that the costs of Cuba's revolutionary experience have been exceedingly high in terms of human suffering and political repression. Moreover, he finds that the regime's economic performance has been nothing less than dismal when measured against prerevolutionary benchmarks, let alone when measured against the regime's own ambitious economic goals. Thus, he concludes that under Castro "the consequences to Cuba has been an almost unmitigated economic disaster. . . . [Because] aside from fishing, there was not a single item in the range of agricultural and livestock products which after twelve years equalled or surpassed the per capita production of 1958" (pp. 196–197).

Whether Cuba's revolution can in fact be "measured" solely according to Nelson's criteria is another matter, however. To begin with, there are obvious methodological problems that involve the reliability, comparability, and significance of statistical data drawn from both periods in which methods of accounting and reporting differ greatly. Furthermore, a twelve-year time-frame may indeed contain a record of "unmitigated economic disaster," but revolutions after all are not made solely for economic reasons, and initial economic downturns are certainly predictable outcomes of revolutionary upheaval. Finally, what one attempts to "measure" may itself be influenced by one's understanding of the genesis and development of the Cuban Revolution. For Nelson, the "old Cuba" was clearly enjoying some economic and societal improvements prior to 1959. Given this perspective, not only was there little objective necessity for the radicalized revolution that followed, but also that revolution became highly dysfunctional when measured against particular indices of past development. Consequently, there is a built-in bias in what Nelson has chosen to emphasize and measure. The results of his analysis are further skewed, in turn, because of his pronounced tendency to understate the political, generational, and nationalist tensions as major determinants in the Cuban phenomenon. These aspects, in short, are not fully accounted for in his appraisal.

The first two chapters dealing with the development and consolidation of the revolutionary process, for example, barely touch on the discredited image of Cuba's old political class owing to its identification with corruption and *entreguismo* (national sellout). As a result, there is little analysis of why Castro's generation of 1953 looked upon itself as the only redemptive force in Cuban political life—at war with not only the Batista dictatorship but also the older political generation. Moreover, Nelson underplays the effect of frustrated Cuban nationalism, the legacy of U.S. interventions and economic-political penetration, and Washington's later policy toward Castro in influencing the course of revolutionary development. Thus, hardly any significance is attached to the 1933 Revolution (pp. 4–5)—specifically, the role of the 1930 Generation, the role of Washington in bringing down the revolutionary government, and the repercussions that the failure of both the 1930 Generation and the 1933 Revolution had on Castro's later behavior. Similarly, Washington's ambiguous if not hostile posture toward the Castro regime in 1959 is depicted as "hardly . . . a decisive factor" in shaping Cuban policy, even though it "reenforced the attitudes which . . . were generated during a half century of experience" (p. 24). In brief, Nelson builds his case against the Castro regime by de-emphasizing precisely those factors in Cuba's pre- and post-1959 situation which evidently elude his understanding or empathy.

As a historical account, Nelson's study is additionally flawed by several factual errors: Cuba's War of Independence (instead of "rebellion") began in 1895 and not in 1896, while U.S. intervention itself prevented the Cuban struggle from being allegedly "successful" (p. 4). Batista's army hardly fits the criteria of a "professionally trained and led army" (p. 14). To claim that "such wholesale and vengeful killing as occurred in Cuba [in 1959] was without parallel in the history of Latin America" (p. 19) simply ignores the Mexican Revolution. Castro did not personally head the contingent that arrested Major Huber Matos (p.

21), nor was the latter arrested in May 1959 (p. 25); and Felipe Pazos of the National Bank rather than Manuel Fernández resigned from the Cabinet in November 1959 (p. 27). Finally, the Soviet trade agreement of 1960 provided for the purchase of 4 million tons of sugar over a 4 year period rather than 5 million tons over a five-year period (p. 27), while the Second Agrarian Reform was in October 1963 and not in October 1959 (p. 73).

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Yugoslavia. By Stevan K. Pavlowitch. (New York: Praeger Publishers, 1971. Pp. 416. $11.00.)

Realizing the difficulties of writing a compact work on South Slavs, Stevan K. Pavlowitch, with a rare combination of erudition and modesty, has condensed past and current political, economic, social, and religious developments in Yugoslavia into a comprehensive book that will be very useful to all those who are searching for facts rather than fancy, and who are interested in reality rather than sensationalism. Using the method of "geometrical progression through history," he puts the developments in the area into a historical perspective.

The author of the book says that he has been, on occasions, irritated by "the fashionable vulgarizers, ignorant myth-makers, pontificating would-be experts, pompous parrots, and all those who dash off a new book simply to exploit the market, after a visit here, an interview there, and with only a shaky knowledge of the languages involved" (p. 21). He cites several examples of distortions and outright falsifications of historical events in the country which, because of the developments there in the 1940s, have captured the attention of a great many publicists ever since.

Pavlowitch does not indulge in polemics, but he presents the developments in the area inhabited by South Slavs in a straightforward manner, destroying along the way many myths and legends—some manufactured in Yugoslavia, some in the West. Condensing the long history of South Slavs from its beginnings to World War II into some seventy-five pages, he examines the factors which brought the present-day political system in Yugoslavia into existence. With brevity and clarity of analysis, Pavlowitch discusses the various phenomena which are usually described as Stalinism-Titoism, Titoism, monocratism, and polyethnism.

In view of the unavailability of documents still classified by the Soviet and Yugoslav governments, the whole story of the much-examined Tito-Stalin break in 1948 will not be known for some time. Being aware of the lack of hard evidence, Pavlowitch resorts merely to pointing out the verifiable facts and leaves the detailed and more meaningful interpretations of those events to future historians who may be able to document the motives of the main actors in the Balkan drama; he follows a similar pattern when he describes the Tito-Soviet cooperation in the 1950s and 1960s. His analysis of Tito's dilemma—adherence to Marxism-Leninism versus desire for independence—comes out very clearly as does the issue of the Tito cult promoted inside the country by the Party and outside of it by some intellectuals in the West. He believes that the survival of the regime created largely by Josip Broz-Tito and his coworkers will depend, to a large extent, on the survival of the cult of Tito.

The book provides the reader with comprehensive information on, and analyses of, many relevant issues such as the traditions in both the Catholic and the Orthodox Churches which have strengthened the centrifugal tendencies in Croatian and Serbian nationalisms; the unemployment problem that forced the regime to permit a million Yugoslavs to earn a living abroad by the end of 1970; the effects of tourism which earns foreign currency for the state and, at the same time, exposes the population to foreign influences; the effects of ideology on the older and younger generations; Tito's censorship of the "counter-revolutionary attempt" in literature and arts; the reasons for the adoption of federal division in state administration; the development of Yugoslavia into a land of some fifteen different nationalities; and the orientation of the generation that will attempt to assert itself when Tito dies. Pavlowitch believes that while the generational conflict in the 1940s played into the hands of Tito more than it worked against him, the present generational conflict, twenty years later, is more anti-Party in its effects; and the political trials in Yugoslavia in the 1970s seem to bear out this contention. Indeed, the Tito regime has openly acknowledged the domestic opposition generated by some intellectuals and writers, by emigrés and churches, by the conflicts of generations following the traditional Balkan pattern, and by the unsolved problem of intermingled nationalities in the multinational state.

The work of Pavlowitch brings into focus the unsettled problem of the several national identities, the negative aspects of nationalisms, interethnic antagonisms and their effect on the ruling party. The rising nationalisms—particularly of the Croats and the Serbs—and the traditional conflict between them have led to speculations that the country will fall apart after Tito dies, but Tito has denied that this would happen. What kind of change will take place in the country following Tito's death is anybody's guess. As Pavlowitch sees it, after Tito's departure the Soviet Union will not allow the situation to deteriorate to its own disadvantage.

This is a scholarly, sober and well-researched book written in a forthright style; and those students of the area, and of comparative politics and international relations who recognize the link between the past, present and future will appreciate this excellent historical reference source.

JOSEF KALVODA
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Nuclear Politics: The British Experience with an Independent Strategic Force, 1939–1970. By Andrew J. Pierre. (London and New York: Oxford University Press, 1972. Pp. 378. $18.75.)

Nuclear Politics is a good account of the evolution of the British nuclear force and doctrine. Better focused than William Snyder's *The Politics of British Defense Policy, 1954–1962* (Columbus, Ohio: Ohio State University Press, 1964) and more thorough (although less analytical) than Richard Rosecrance's *Defense of the Realm* (New York: Columbia University Press, 1968), it can be profitably read either as an introduction to the subject or as a memory refresher and should serve as the standard treatment for some time to come.

Since it covers thirty years, the book cannot cover in detail any of the interesting events. And because the author chooses to stick close to the history of the period, he leaves to others the job of formulating and testing propositions with interesting theoretical implications. If the book has a theme, it is the familiar one that "The British nuclear force has been as much a product of politics, economics, military, scientific, and bureaucratic interests, pressures, and perspectives as it has been of strategic analysis and theoretical logic" (pp. 311–12). The role of domestic politics, including the sometimes divergent pressures of public opinion and party opinion, is well documented. Several weapons systems would have been canceled earlier, with less cost and less harm to the defense establishment, had the government not developed a political stake in them (p. 310). Public support for an "independent" nuclear force has remained fairly steady and fairly high, but a greater constraint on the decision makers has been the views of party activists—a strong group of nationalists in the Conservative party and a group of unilateral nuclear disarmers in the Labour party. Both wanted Britain to play a role independent of America. While neither wing triumphed, each had influence. The former strengthened the hand of Conservative governments in negotiating with the U.S., inhibited the consideration of alternatives to the British force (e.g., merger with a French or with a wider European force), and led Conservative governments to exaggerate the degree to which their deterrent was in fact independent, thereby hindering realistic analysis of Britain's position. The unilateralists temporarily succeeded in putting the Labour party on record as renouncing nuclear weapons and pushed Wilson into the fuzzy pledge that he would "renegotiate" the Nassau agreements if the Labour party gained power in 1964.

Policy was debated within a setting of three basic dilemmas that neither party chose to confront directly. In 1948 Churchill described Britain as "at the very point of junction" of three "majestic . . . interlinked circles"—the Commonwealth, the English-speaking world, and "United Europe." The problem, of course, is that activities in these arenas, while bringing prestige and other benefits to Britain, often conflicted with each other, especially when there were strong demands for using scarce resources for domestic pursuits. Similar dilemmas of choice operated in the trade-offs between nuclear and conventional forces. Although the Conservatives stressed the importance of an independent nuclear force they would not spend the sums necessary to procure it. And while the Labour party talked of the role of conventional armies in world politics it would not pay the high domestic price of re-instituting the draft. The initial attempt to be strong in both kinds of forces led to weakness on both fronts. Thus Pierre points out that the stress on an independent nuclear deterrent which appeared in 1954 and culminated in the 1957 White Paper preceded by several years the development of a viable nuclear force. The third dilemma was posed by the fact that although Britain sought both independence from the U.S. and influence over it, the paths to these two goals often diverged.

These considerations, which run through Pierre's book, undercut the quotation from Zuckerman in the frontispiece: "The decisions which we make today in the fields of science and technology determine the tactics, then the strategy, and finally the politics of tomorrow." At almost every point—from the decision to make the bomb, to the decision to cooperate with the U.S. insofar as the U.S. would allow it, to develop an independent capability when the McMahon Act cut off information, and to maintain a nuclear force of limited military utility—politics came first. Of course there were unintended technological consequences. Perhaps the most important one was that, as Pierre points out, by trying to match the superpowers in too many areas of military technology Britain spread her resources too thin, failed badly (e.g., the TSR-2 aircraft, the Blue Streak missile), and gave up. Had she been willing to do less, or move more slowly and thereby take advantage of the American experiences, she could have ended up with a cheaper and more independent force, as France did. But even here while technology partly explains the cost of trying to be a major power, politics, especially domestic politics, explains the choices that were made—often inadvertently.

And, in the last analysis, the defense policy produced by the political pressures may not have been so bad after all. The "erratic pattern of weapons acquisition" (pp. 320–21) certainly wasted money, but the foreign policy consequences were limited. A truly independent nuclear force would have been useful mainly as a symbol (which Britain needed less than France) or as a deterrent against a Russian nuclear attack directed at Britain alone (a negligible danger). A large conventional army might have been more valuable, both in gaining support from other European states as a reciprocation for a greater British contribution to NATO and in protecting British interests in the Third World, but it is questionable whether the gains would have been worth the price of continued conscription. The more significant price may have been domestic. Some inconsistency and vacillation was an almost inevitable consequence of the painful loss of power in the world. But as a result of domestic politics and the absence of courageous and creative leadership, these disconcerting weaknesses were magnified, thus helping to undermine British self-esteem and confidence.

ROBERT JERVIS

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Conservation in the Soviet Union. By Philip R. Pryde. (London and New York: Cambridge at the University Press, 1972. Pp. 301. $14.50.)

Professor Pryde has done a first-rate job of researching, organizing, and writing a cogent account of the most important aspects of conservation in the Soviet Union. For so brief a book, the coverage is indeed comprehensive, and yet about as detailed as it can be, given the limited amount of factual material that is available to Western writers. Only occasionally might the author have been able to update his information a few years, and those omissions are probably explained by the lag between the writing and the publication of the book. The writing is superb, smooth and organized so as to support the development of basic concepts that lead logically from one thought into another.

The selection of topics reflects the desire to present a complete picture of conservation in the Soviet Union, ranging all the way from basic Soviet precepts to actual practices and problems in some of the more significant areas of the environment. After a brief introduction which provides insight into Soviet rationale regarding conservation, the author provides a fairly comprehensive chapter which documents the development of the conservation movement in Tsarist Russia and the Soviet Union. This is supported by lengthy appendices in the back of the book which include, among other things, excerpts from the 1960 Law on the Conservation of Nature in the RSFSR; recommended outlines for courses in conservation in higher educational institutions in the Soviet Union; the text of the 1968 USSR Land Legislation Act; the 1921 Decree of the Council of People's Commissars on the Preservation of Natural Monuments, Gardens, and Parks; the Text of Water Legislation Principles, and so forth.

These accounts of principles and historical trends are followed by chapters on conservation of Soviet land and soil resources, the *zapovedniki* (nature preserves) in the Soviet Union, management of fisheries and wildlife, the extraction and conservation of timber and mineral resources, water resource utilization and conservation, and environmental pollution and environmental quality in the Soviet Union. These chapters are sprinkled with case studies of some of the better-known conservation problems in the country, such as the Caspian Sea problem and the Lake Baykal controversy, and are further supported by additional appended materials on endangered wildlife species, statistics on rivers and other water resources, and so forth.

The last chapter summarizes attitudes, problems and trends in conservation in the Soviet Union, and happily includes brief accounts of Soviet theory and practice toward population without which a study of conservation could not be complete.

Thirty-one pages of bibliography attest to the thoroughness and balance of the author's research. In addition, the many homely insights reveal a first-hand intimacy with the Soviet scene.

In short, I have read the book from cover to cover and have found nothing particularly wrong with it. This is my highest form of praise. The book is a timely piece that serves as a nice companion volume to Marshall I. Goldman, *The Spoils of Progress: Environmental Pollution in the Soviet Union* (Cambridge, Mass. and London: the M.I.T. Press, 1972, pp. 372). The two books cover some of the same ground regarding Soviet attitudes toward conservation and historical background of the development of the movement in the country. But Goldman's book is more narrowly directed toward pollution problems, whereas Pryde attempts to span the broader field of conservation in general. And, of course, Pryde's book is written from the geographical point of view, while Goldman's book is more an economic treatise. The two books together provide an excellent conceptional and factual account of environmental problems in the Soviet Union and governmental reactions to them.

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Dictatorship and Armed Struggle in Brazil. By João Quartim. Translated by David Fernbach. (New York: Monthly Review Press, 1971. Pp. 250. $6.95, cloth; $3.45, paper.)

Quartim sets three related goals in this volume: (1) to describe in Marxist terms Brazil's class structure in the present period of dependent capitalist development; (2) to analyze and draw lessons from the experience of the armed revolutionary movement of the late 1960s; and (3) relying on the foregoing analyses, to propose a series of tactics best suited to overthrowing the military government and ultimately establishing a socialist regime. As a polemic based on "intimate knowledge" (cover flap) of the armed opposition movement, it succeeds far better in the latter tasks than in the first.

Of the two major tendencies in the contemporary Brazilian revolutionary opposition movement, described here as the militarist and the Leninist, Quartim casts his lot with the latter and castigates the militarists for an adventurism destructive to the entire movement. He criticizes them for rejecting traditional Marxist-Leninist methods of building a mass movement and for reducing organizational structure solely to the armed groups advocated by Regis Debray in *Revolution in the Revolution?* Detailing several operations undertaken by militarist groups which brought disastrous results for the revolutionary opposition, he accuses their leaders of "forgetting that the annihilation of the enemy's armed forces required the preservation of [their] own" (p. 212), and he warns that such tactics "may end with the death of an entire generation of revolutionaries" (p. 237). The author also describes how some armed groups, their priorities distorted by their initial success in the preparatory, urban stage of guerrilla warfare, failed to move on to the next, rural stage. Yet only in the countryside can armed opposition be feasibly coordinated with organization-building among the people.

In seeking to refine the Leninist position, Quartim prescribes: (1) a national revolutionary *party* to coordinate the clandestine as well as legal operations while pursuing the revolutionary interests of the working class, and (2) a *front* to enlist multi-class support, based on a minimum program demanding an end to the dictatorship, expulsion of imperialism, liquidation of monopoly capital, agrarian reform, and workers' control of production. To be implemented successfully, such a strategy calls for attracting the urban and rural masses into dynamic organizations by championing their deeply felt grievances against employers, landlords, and the state.

Regarding Quartim's first task, the lengthy historical introduction and hasty analyses of Brazil's socioeconomic structure offer provocative hypotheses but are replete with factual error. He seldom documents his sources, and his statistics of automobile, tractor, and wheat production and of land tenure patterns simply do not jibe with those in Brazil's *Anuário Estatístico,* in some instances through obviously faulty transcription and at other times because he has relied on erroneous secondary sources. Other examples abound.

The historical section reveals a serious misunderstanding of the theory and practice of Brazilian corporatism. Corporative theory was not a "borrowed" accretion to Brazilian jurisprudence. Rather it has constituted a major current of Brazilian political thought in this century. And though the corporative institutions have indeed worked (and still do) to prevent the autonomous organization of the working class, co-optation is a more important mechanism to achieve this than the government imposition of union heads which Quartim alleges. The genius of the corporative system established by Vargas in the 1930s lay in its creating conditions whereby docile and corrupt union bosses emerged from the working class itself.

When the author observes that Vargas's Estado Nôvo failed to survive the end of the Second World War, he overlooks the fact that its basic corporative institutions remained in the new liberal democratic state (1946–1964), enabling political elites to continue controlling the working class. Given the continuity of these corporative institutions, I cannot agree with Quartim that the coup of 1964 meant "the creation of an entirely new state" (p. 53). Quite the contrary, it represents a concentrated effort to make the corporative institutions of the Estado Nôvo work more effectively for Brazil's dominant classes.

Despite his weakness in documentation, the author does advance an interesting hypothesis about the "pact of dependence" between the elites of Brazilian and foreign capital. Countering the argument that since 1964 the military government has dealt a fatal blow to the Brazilian bourgeoisie, he claims that these capitalists, as junior partners of international monopoly capital, expanded their access to markets abroad. For their part in the bargain, giant multi-national firms consolidated their control over heavy industry, and Brazilian state capital received vast foreign credits to expand its activities in the economic infrastructure. Hence the continuing utility of class analysis and dependency theory, for the increasing dominance of foreign capital has lent new vitality to the Brazilian ruling class instead of destroying it.

To conclude, Quartim's slipshod research does not diminish the interest of his hypotheses about recent socioeconomic changes in Brazil. More important, his constructive criticism of the armed opposition movement, backed up by fine descriptive analysis, offers a far better plan for ultimate triumph than Debray does.

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War Comes to Long An: Revolutionary Conflict in a Vietnamese Province. By Jeffrey Race. (Berkeley: University of California Press, 1972. Pp. xxiii, 299. $11.95.)

Lest one think that everything about Vietnam has been said, along comes a major work on the origins and development of the Vietnamese Communist revolution that provides exciting new material for the historian, the student of revolution, and the policy analyst.

Race's book, which examines the conditions of revolutionary war in one Mekong River delta province, offers first of all an original and convincing explanation of the NLF's evolution. Race disputes both the argument that the NLF was always Hanoi's tool and that it developed entirely independently of northern direction. His careful sifting of revolutionary documents and close familiarity with local politics lead him to suggest: that after the 1954 Geneva Conference, the NLF anticipated coming to power when Diem's regime collapsed; that it moved fitfully from political to armed struggle as Diem's suppression campaign took effect; that Hanoi's hesitancy to support armed revolution was a source of great consternation to southern cadres; and that Hanoi finally changed its general directives to the southern movement only when, in 1959, it became apparent that armed revolution was the only recourse to the NLF's disintegration. Thus, the north *did* provide its southern compatriots with strategic guidance, and *did* direct after Geneva that southerners bury their arms for the anticipated day of takeover. But the north consistently, in the latter half of the 1950s, had to respond to the activities of a purely southern movement that was contending with an oppressive government carrying out a reactionary land-[back]-to-the-landlord program. The NLF was independent of external support, though not of external guidance.

War Comes to Long An is equally impressive in its contrasting evaluation of the sources of NLF success and government failure in village leadership. The NLF did well in Long An because it developed and implemented a comprehensive program of social change that directly appealed to peasant interests. Its multidimensional revolutionary strategy included a stronger and more locally relevant organization and an attractive program that emphasized the redistribution of wealth and status. Race's analysis is a useful antidote to those rather superficial models of revolution that exaggerate the importance of "insurgent" organization.

By the same token, his book is useful in providing analytic tools for critically assessing American counterinsurgency tactics, such as the Phoenix operation, which stressed violent methods of uprooting the so-called Viet Cong infrastructure. The basic failure of such programs lay in the American and South Vietnamese inability to comprehend the NLF's social revolutionary nature. Race makes a solid case that no government strategy based on new tactics, new weapons, or even a new distribution of resources could win. Only a strategy that simultaneously instituted radical social policies and refrained from responding with violence to the opponent's successes could hope to compete with the NLF.

The author leaves it to others to extrapolate from the Long An experience to the rest of Vietnam, from there to the rest of Southeast Asia, and possibly even to revolution and intervention throughout the Third World. But the implications are strong enough: Long An is revolution and counterrevolution in microcosm, wherever the forces of rapid change confront entrenched power and privilege backed by foreign guns.

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The Sword and the Scepter: The Problem of Militarism in Germany. By Gerhard Ritter. Translated by Heinz Norden. (Miami: University of Miami Press, 1969): Vol. I: The Prussian Tradition, 1740–1890 (338 pp. $15.00). Vol. II: The European Powers and the Wilhelmian Empire, 1890–1914 (328 pp. $15.00.)

It may, at first blush, seem odd to recommend that these two volumes be read in conjunction with David Halberstam's *The Best and the Brightest* or other literature on the Vietnam problem, but the suggestion is not illogical. Gerhard Ritter's magisterial study was undertaken in the midst of the Second World War in order to help answer a question that was troubling many of his countrymen, namely, "How did the German people, for centuries among the most peaceful nations of the west, come to be the terror of Europe and the world, acclaiming a violent adventurer who will live on in history as the destroyer of the old European order?" This translation of the first two volumes of his work appears at a time when many Americans are asking similar questions about their own country.

Ritter's answer, most historians would agree, was only a partial one (he was a historian of the old school, with little interest in the sociological and psychological aspects of his subject), but for that very reason, it is all the more relevant to our own problems. His book was called *Staatskunst und Kriegshandwerk,* which, properly translated, means, not "The Sword and the Scepter" (a fatuous title) but "The Art of Statesmanship and the Craft of War." His thesis was that throughout the history of the great states of the West, there has always been tension between the two—an uneasy relationship that is the consequence of the conflicting views of their practitioners. The statesman, if he is true to his calling, appreciates the uses, indeed, the indispensability, of military power but insists that it must be an instrument of policy. The soldier, while paying lip service to this principle, will in matters of practice resent its application. In time of war, the statesman's job is "to curb its internal dynamics, to shorten it, to limit its aims . . . to keep war instincts . . . from getting the better of calm political reason" (I, 9). But it is particularly in wartime that the soldier is most resistant to restraints. He is always "against neglecting any occasion to destroy the enemy forces," whereas "the statesman may fear destruction of permanent values, undesirable political side-effects, the stiffening of the enemy's resolve against peace" (*Ibid.*). Of the two, it is generally the soldier who has the greater self-confidence, because his view is narrower (Ritter would have been interested in Halberstam's statement about U.S. commanders in Vietnam, namely, that "their particular certitude made them more powerful players than men raising doubts"); and the statesman, therefore, is always in danger of being overborne by the brasshats and of submitting to arguments of military expediency.

In German history this problem was endemic and ultimately disastrous, and, before he died, Ritter had finished four large volumes on the evolution and resolution of the civil-military conflict. Of these, the first two, now ably translated by Heinz Norden, are the most satisfactory, for the later volumes, which deal with the First World War, are filled with polemical sallies against the Fritz Fischer school of history, and the author sometimes loses his sense of perspective. In contrast, those dealing with the eighteenth and nineteenth centuries and the origins of the First World War are both more objective and more readable, free from the mountains of detail that Ritter piled up in later volumes to protect himself against counter-assaults from the Fischerites.

In the first volume, for instance, after a discussion of the power politics of Frederick the Great and the development of the techniques of limited war in the eighteenth century, Ritter describes the revolution in warfare that took place in the Napoleonic period and the work of the great theorist who first recognized its significance. It was Clausewitz who had the wit to see that, with the coming of the mass army and the new technology, war would constantly tend to assume its absolute forms, and it was he who handed down, less as a definition than as a goal, the dictum that "war was [i.e., should be] the continuation of state policy by other means." Clausewitz was no mere theorist. He had crossed Europe with the Russian armies that pursued Napoleon from Moscow, and at the end of that journey, as a soldier in Paris in 1814 and 1815, he had had ample opportunity to watch the Prussian commanders Blücher and Gneisenau attempting to take policy into their own hands. That civil-military conflict was the forerunner of the many that followed, particularly those between Bismarck and the Prussian military during the war years 1864, 1866 and 1870–71, which are discussed with authority and rich detail in the second half of this volume.

In Bismarck's career, Ritter holds, "the phenomenon of true statesmanship may be studied . . . to better effect, than in almost any other figure in our history" (I, 11), for in the Chancellor's person national self-assertion and a sense of European responsibility were combined, and while he respected military power, he used it with restraint and for precise political objectives. To him, as to Clausewitz, war had its own grammar, but not its own logic; the latter was supplied by the statesman. Such views did not ingratiate Bismarck with Moltke and his "demi-gods," and the Chancellor's whole career was marked by military attempts to throw off his restrictions and, increasingly, to encroach upon his sphere of competence. These he was able to defeat, but his successes in this respect were the last for the civilian principle.

How true this was Ritter demonstrates in his second volume, which begins with a brilliant section describing the army's role in politics in France, Britain, and Russia in the years 1890–1914 (and showing that none of these countries was free of military encroachment upon decision making). It continues with a discussion of the factors in Germany that made this more dangerous there than in other countries—such things as the progressive militarization of the thinking of the middle class, the dominance of Social Darwinism, and the lack of civilian political talent after Bismarck's fall. This brings the author to the culminating sections in these volumes, those on Tirpitz and the growth of the battle fleet and on Schlieffen and the ascendancy of the General Staff. Here he provides brilliant descriptions of the way in which, in moments of perplexity and danger when the international order begins to crumble and crisis is the order of the day, the terrible simplifiers in uniform can take over and, with their breezy self-assurance, commit the nominal leaders of the country to courses pregnant with disaster.

GORDON A. CRAIG

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Political Parties and Pressure Groups in Britain. By Geoffrey K. Roberts. (New York: St. Martin's Press, 1972. Pp. ix, 203. $8.95.)

Dr. Roberts, who lectures in political science at the University of Technology, Loughborough, England, has written an introduction to the subject of political parties and pressure-groups, "those influential organizations which do so much to shape the decisions which the more formal of our [British] political institutions make for us" (p. ix, introduction). In the first of his nine chapters he sets the scene with a virtual justification for presenting the remaining eight. It is that the individual citizen, feeling he has "less influence on the shaping and direction of public affairs, than was possessed by his nineteenth-century counterpart" regains ground through these intermediate groups, which "filter and focus the multiplicity of demands that the citizens have on government . . ." (p. 7). In so doing the groups do not subvert democracy, they complement it.

Distinguishing in the conventional way between parties and pressure groups, Dr. Roberts then devotes three chapters to the former, one specifically to the latter (which are equated with lobbies and interest groups). Chapter 2 is given over to the history of (English) parties, pre-1832 (pp. 14 through 18), post-1832 (pp. 18–28). Chapter 3 opens with a specification of the functions of parties that follows traditional lines. The chapter is rounded out with a sketch of the anatomy of British party organizations and party programs. Chapter 4 deals with leaders, generally meaning front-benchers in Parliament, and followers. Having touched on the qualities required of leaders, Dr. Roberts proceeds to ask: Who are they? How are they selected? Followers are divided into activists, supporters, and sympathizers. The relationship between leaders and followers is then explored. Predictably, Michels is brought on stage, and his hypothesis is discussed along four dimensions; "the appointment and change of leaders, the distribution of power within the party, the way in which decisions are made, and the sources and control of finance" (p. 69). The broad conclusion is that "in all three British parties, effective control is, and to a large extent inevitably must be, concentrated in a few hands—generally the Leader, the chief members of the parliamentary party, and a small number of top executives in the central bureaucracy . . ." (p. 77).

Turning to the other species of intermediate groups, the author distinguishes between lobbies (specialized in one political role) and pressure groups, conceived as having other functions as well. Their history is then sketched (pp. 81 through 89), after which Dr. Roberts has something to say about their objectives and rather more about their methods and their organization. The conclusion of this part of the book is that (in Britain) there "is a kind of 'countervailing power' in the exercise of political, as well as economic, influence" (p. 106). Accordingly, pressure groups are judged "politically beneficial" as well as, by virtue of stimulating public debate, "an educative force."

With Chapter 6 (Groups and Policy), wider considerations are introduced. A "ladder of values" is identified. Its bottom rung is made up of ideology (a set of basic values), from which policies extend, and, higher still, political decisions. Party is related to the ladder of values through the 1966 Election manifestoes, from which the relationship between ideology, policy, and program is extracted. This is followed by a brief attempt to illustrate the dynamics of the relationship in practice. Pressure groups come on this scene as vehicles for public opinion on particular issues (apparently to be equated with policies on the ladder of values), although they also express ideologies.

If ideology provides "the fuel for the vehicle of their political behavior," the parties and pressure groups do not run free, i.e., the vehicle does not lack "brakes." Such "constraints" are the subject of Chapter 7. These arise principally from within the parties or pressure groups, the political system, another of the subsystems, such as the economic; or from another State. In Chapter 8 the discussion becomes comparative—an essentially structural-functional treatment based on a somewhat idiosyncratic classification of political systems. The concluding chapter pulls the threads together in terms of "the plural society," where "no exclusive source of basic values" is recognized and free competition is invoked for reaching acceptable compromises. This competition is represented mainly as occurring between parties, but "often" as encapsulating the claims of pressure groups, which also, however, go their own way. It follows that within the political system, there is a place for conflict as well as consensus, and even consensus ought not to be so interpreted as to overlook the claims of the *un*organized. Finally, Dr. Roberts asks, "Can the plural society which Britain has become, with its paraphernalia of political parties and interests and pressure groups, be called a democracy?" (p. 190). Answer: "political parties and pressure groups perform many functions which help to carry out" the "requirements of democracy" (p. 191).

The book is what it purports to be—an introduction. It could be safely put in the hands of freshmen and sophomores for courses on British or comparative politics. But American instructors would have to be prepared to identify more fully a number of the bit players (e.g., Desmond Donnelly, Woodrow Wyatt) who walk on this political stage, and to explain some of the brief historical references (e.g., Catholic Emancipation, the 1832 Reform Act). Instructors will not need any encouragement to question some of Dr. Roberts's judgments. One cannot, for example, *deduce* "countervailing power" from the mere existence of "natural opposites" or even from the free play of debate in which "both sides" are presented (pp. 106–7). Nor is the Schattschneider criticism weighed in British terms—that in the pluralist heaven, the chorus sings with a strong upper-class accent. All things considered, however, this is a useful book for the younger student.

GRAHAM WOOTTON

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Brazil in the Sixties. Edited by Riordan Roett. (Nashville, Tenn.: Vanderbilt University Press, 1972. Pp. xvii, 424. $15.00.)

Modern Brazil: New Patterns and Development. Edited by John Saunders. (Gainesville: University of Florida Press, 1971. Pp. x, 350. $12.50.)

Brazil: People and Institutions. By T. Lynn Smith. Fourth Edition. (Baton Rouge: Louisiana State University Press, 1972. Pp. xx, 778. $15.00.)

The early 1970s have proved to be banner years for the publication in this country of reasonably rigorous social science research concerning Brazil. Always one of the world's largest countries, this emerging "Colossus of the South" has very recently entered the select company of nations with more than 100 million inhabitants and the equally restricted circle of those industrial powers boasting a GNP of over $50 billion. In this context, comprehensive assessments of the socioeconomic, demographic, and institutional aspects of contemporary Brazil are particularly timely, even if they are based upon data and research of the 1960s. When such studies represent fairly sophisticated analyses of highly qualified scholars, as in the volumes under review, political scientists ought to pay serious attention to the insights they may provide regarding the process of modernization and development in a country edging close to attaining continental paramountcy.

Only one of these three broad-ranging works purports to treat political matters directly and at any length. Roughly the first quarter of the symposium edited by Roett includes a workmanlike review of political developments from 1946 through 1969 by the editor, now an Associate Professor of Political Science at Vanderbilt University, and an uneven summary of Brazilian-United States relations by Peter Bell, who during most of the 1960s was a representative of the Ford Foundation in Brazil. Sandwiched between is by far the most intellectually satisfying portion of these three books, at least from a disciplinary and conceptual point of view. Douglas Chalmers, Associate Professor of Political Science at Columbia's Institute of Latin American Studies, has provided us with an insightful essay on "Political Groups and Authority," appropriately subtitled "Some Continuities in a Decade of Confusion and Change" (pp. 51–76). Focusing upon the "lack of an acceptable conception of the basic character of the Brazilian political system" Professor Chalmers explores the "enduring political structure" underlying that country's formal institutions. In stressing the importance and persistence of essentially elitist "vertical" relationships formed around authority and domination as part of a "changing, dynamic, and distinctive" political style, he rejects the pervasive notion that they are transient characteristics of a traditional or transitional stage in the development process. Quite validly he questions "whether class or any other basic cleavage accurately describes the differences between Brazilian power contenders of any period" (p. 65). Underscoring the "hyper-fluidity" of political support at the national level, Chalmers suggests that in the Brazilian case the conventional dichotomy between the "técnico" and the politician is increasingly meaningless as the new politician-technician becomes the crucial type of political actor. He is thus able to conclude that

> there are no grounds for assuming that Brazil will or must establish a pluralistic, liberal-democratic form of government in order to become more developed. If democratic values of responsiveness to citizen demands, egalitarianism, and provisions for participation in politics are to be achieved, they will not come through copying older democratic institutions appropriate to another sort of politics in another age. They will have to emerge out of efforts to shape new types of vertical groups and new relationships between such groups and the increasingly technical political process (p. 76).

By contrast with the Roett symposium, that edited by University of Florida sociologist John Saunders includes only a very formalistic and traditional paper on "The Political and Legal Framework of Brazilian Life" by Anyda Marchant which emphasizes the legal façade over the political realities. The first twenty pages of Senhora Marchant's essay do, however, furnish a brief sketch of Brazilian political history useful for orienting the uninformed reader. For those who need such orientation, this essay should be read as background to the Roett volume. Indeed, the same holds true for Donald Dyer's succinct discussion of geographical factors (pp. 29–50) and J. V. Freitas Marcondes's paper (pp. 133–55) treating "The Evolution of Labor Legislation in Brazil," since he deals with this topic in the broader context of the evolution of the Brazilian governmental system from the mid-sixteenth century to the present.

If the Roett book is the stronger on political topics, Saunders excels in the analysis of societal factors. The editor's own essay on "The Modernization of Brazilian Society" is a useful overview of the subject, and its discussion of social differentiation and cultural integration prior to 1964 adds depth to the other book's interpretation of political trends. To a considerable extent this could also be said of two other sociological papers in the Saunders volume: Sugiyama Iutaka's "The Changing Bases of Social Class in Brazil" and José Arthur Rios's examination of "The Growth of Cities and Urban Development." Cultural matters of possible political import are covered by Earl W. Thomas in "Protest in the Novel and Theater" in Roett and "The Evolution of Brazilian Literature" in Saunders. The latter provides a background survey, while the former is of greater use to the political scientist concerned with the present situation. Its companion essay on the short story by Alexandrino Severino falls between Thomas's two chapters in terms of its relevance to the student of Brazil's politics.

Economic themes loom large in both the

Roett and Saunders books. In the former, Werner Baer and Isaac Kerstenetzky furnish an account of the Brazilian macroeconomic scene depicting the 1960s as a decade of adjustment to the structural changes of the 1950s. Their projections for the 1970s, however, err substantially on the side of caution, with the per capita GNP predicted for 1980 in fact likely to be reached by 1975 instead. In the Saunders symposium, Eric Baklanoff covers much of the same ground in greater historical context, but fails to make any prognostications for the 1970s. Although in both collections William H. Nicholls deals with the agricultural sector, his more current analysis and policy prescriptions appear in the Roett-edited volume. Highway expansion is singled out as the major catalyst of change in rural areas by this long-time observer of the Brazilian scene. The Roett work also includes a useful essay on the economic functions of the public sector by Andrea Maneschi and a detailed technical study of tax credit impact on industrialization in the Northeast, while the Saunders volume includes a general historical sketch of industrialization prior to 1964.

Each of these books devotes some attention to the rapidly changing field of education, although neither brings the reader fully abreast of the present situation, particularly in the field of higher education. The late Anisio Teixeira analyzes "The Changing Role of Education in Brazilian Society" for the Florida symposium and Douglas Graham surveys "The Growth, Change, and Reform of Higher Education in Brazil" for its Vanderbilt counterpart. Complementary much more than overlapping, because the philosophical Brazilian educator's deeper time perspective provides an excellent backdrop for the recent data amassed by the technically oriented young U.S. economist, these essays are of substantial importance to anyone hoping to understand Brazilian modernization.

T. Lynn Smith, Florida's distinguished Graduate Research Professor of Sociology, contributes to the Saunders study a brief essay on "The People of Brazil and Their Characteristics." He has subsequently published a fourth edition of his classic, *Brazil: People and Institutions* (which first appeared in 1946). Rather than a complete revision, this is a reprinting of the 1963 edition with the addition of a lengthy supplement covering the more recent trends and data (pp. 627–730). But given the author's more than thirty years' systematic quest for understanding of the processes at work in Brazil, these meaty chapters are a must for any scholar concerned with the societal roots of Brazil's political forces. For while the data may be available elsewhere, Professor Smith is specially gifted in their use and making wise inferences from them. Thus his meticulous demographic analyses indicate that with an end to large-scale immigration and the falling of birth rates among the middle class, the long observable racial "bleaching" will be reversed during the 1970s; that while real differences between urban and rural life are being reduced, the perceived deprivation on the part of the rural population is still on the rise; and that the Trans-Amazonian highway "could mean fully as much for the agricultural, industrial, and commercial development of the northern part of South America as the Aswan Dam will for northern Egypt" (p. 651).

Great improvements in farming methods, promising beginnings in planned colonization, and the manifold impact of rapid sustained urbanization are among the themes explored by Professor Smith. He argues persuasively that many of the societal changes observable in contemporary Brazil parallel those in the United States during the 1920s. His conclusions on changing class structure, family life, and community practices have implications for the political scientist greater perhaps than do the frequently decreed modifications in governmental institutions. With respect to politics proper, Professor Smith makes but a few random observations in keeping with his general thesis that so much has occurred so rapidly that "it probably would have been almost impossible for anyone, even a specialist working full time on the study of Brazilian political affairs to have kept fully informed about these happenings" (p. 726).

In sum, these three works do not provide a balanced and comprehensive treatment of Brazil's political panorama. They do furnish much of the understanding of Brazilian society and the industrialization process essential to any adequate comprehension of that country's political affairs. A just sufficient summary of political trends is furnished for those readers concerned with locating Brazil in relation to the developed and developing nations. The Brazil of 1973, owing in large part to economic growth rates averaging nearly 10 per cent annually for the past five years, has already moved beyond the limits suggested in many of these essays. In the political realm, however, it must still cope with the problems so concisely posed by Chalmers. Otherwise, the net yield of these three studies for the student of politics is substantial, albeit largely indirect.

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Youth and Authority in France: A Study of Secondary Schools. By William R. Schonfeld. (Beverly Hills, Calif.: Sage Publications, Inc., 1971. Pp. 80. $2.80.)

This short monograph, published as a Sage Professional Paper, represents a potentially significant contribution to the literature dealing with political development in France. The author has two goals: (1) to examine empirically the authority relationships in French secondary schools; (2) to consider the political implications of the style of authority, in particular, as it affects political development.

Professor Schonfeld reports that authority relationships in French secondary schools follow a developmental model alternating, in the lower grades, between two patterns labeled the *authority-laden syndrome* and the *chahut* (classroom insubordination) *syndrome*. The first is characterized by a high degree of compliance by the students to the directives of teachers, the second represents a status of rebellion against particular teachers. The apparent factor deciding which pattern will occur is labeled the degree of *Caesaristic legitimacy*.

The socialization process over the secondary-school cycle results in two types of authority relationships. In the first place, the early cycle of alternating patterns may persist if the teacher is not perceived as facilitating the achievement of expected goals which include learning and passing exams. In the second place, Schonfeld suggests that when goals are met, authority relationships follow what he labels the *assumed-coverage* cycle, that is to say, a pattern of authority characterized by the students' internalization of authoritarian directives. In reality, however, such directives are assumed since they are not actually issued by teachers.

The significance of the monograph lies in the author's attempt to relate his findings to the political system, in particular, to the pattern of political change in France. Schonfeld suggests that political authority in the French system may follow the pattern outlined in secondary schools. Thus he argues that the usual form of government in France is of the *assumed-coverage* type under which citizens comply with a set of assumed directives which are internalized rather than issued by the authorities. This form of government will persist as long as three requisites are met: (1) The level of economic output sustains a reasonable standard of living; (2) internal stability exists; (3) foreign military involvement does not threaten the internal stability of the system. However, Schonfeld argues that when these requisites are no longer met, "there will be a movement away from *assumed-coverage* government toward an *authority-laden* regime in which the political system impinges much more directly on the citizens" (p. 70). It is during the transitional phase that the author suggests that *chahutage* (read political instability) will occur.

The author's conceptualization owes much to Harry Eckstein, in particular, the latter's emphasis on the political significance of the linkage between political and social authority patterns. Schonfeld also draws on Crozier's *Bureaucratic Phenomenon,* while pointing out the limited political implications of that work. Though the author does not refer to Stanley Hoffmann, his model reminds one of Hoffmann's definition of homeorhetic change outlined in his essay on "Heroic Leadership: The Case of Modern France" in *Political Leadership in Industrialized Societies,* edited by Lewis Edinger (New York: John Wiley and Sons, Inc. 1967).

It is too early to assess Schonfeld's contribution since the present monograph represents only the first step in a larger project in which the author intends to examine the pattern of authority in other sub-systems. Only then will the model be fully articulated and available for general testing. One can only hope that the final product will not be very far off.

J. P. RICHERT

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The Long Revolution. By Edgar Snow. (New York: Random House, 1972. Pp. 269. $6.95.)

Red China Today. By Edgar Snow. (New York: Random House, 1970. Pp. 749. $20.00.)

It is surprising that even Edgar Snow could have misunderstood Chairman Mao. After a conversation with Snow on December 10, 1970, the Chairman courteously escorted his longtime American sympathizer to the door. Mao said that he was not a complex man but really a very simple one. He was, Mao said, only a lone monk walking the world with a leaky umbrella.

Here, the Chairman apparently was using a Hunanese slang expression, *"ho-shang ta-san"* (a monk walking under an umbrella), which was misunderstood by Snow and possibly his interpreter too. It means *"wu-fa wu-t'ien"*: "no law (same sound as "hair" in Chinese) and no sky." In other words, Mao said that he was a man unrestrained by any regulation or law!

Snow also erred more than once when he injected Chinese words into his writing (pp. 24, 180 and 219), which illustrate the pitfalls common to many students who want to show off English. However, his posthumous publication, *The Long Revolution,* not only confirms some of the well-known facts on the Cultural Revolution, but also adds immeasurably to our knowledge of the episode.

It is now confirmed by Mao that for years, Teng Hsiao-p'ing, Secretary General of the Party, did not report to the Chairman. Mao complained that Teng was hard of hearing but at meetings would sit far from him, and that in 1965 Mao could not get the Party-controlled press in Peking to publish an article criticizing an historical play. The article—revised 11 times, we are told, and seen by both Mao and his wife, Chiang Ch'ing—was finally published in a Shanghai newspaper in November 1965. It marked the beginning of the Cultural Revolution.

There are a number of crucial questions that one might wish to have the privilege of asking the Chairman. For example, when did he finally decide that his successor, Liu Shao-ch'i, had to go? We are now told that the moment of decision came in January 1965 when Mao put before a meeting of the Political Bureau a program for the coming Cultural Revolution. Right at that meeting Liu strenuously opposed the program's first point, which was an outgrowth of the Socialist Education Movement. The first point denounced and demanded the removal of "those persons in authority within the Party taking the capitalist road." But it was after the publication in Shanghai of the above-mentioned article that events unfolded rapidly.

According to Mao's comments to Snow, there were two fundamental issues underlying the Cultural Revolution. In addition to "revisionism," a closely linked issue was posed by Liu, who had sought a compromise in the Sino-Soviet impasse when the United States escalated the Vietnam war. Liu wanted to send a Chinese delegation to the Soviet Twenty-third Party Congress to reactivate the Sino-Soviet alliance. Mao resolutely refused to be drawn into a position of dependence, as in Korea, and a possible double cross.

The possibility of a double cross is an important factor to keep in mind when one attempts to explain why, when China finally sought a detente with one of the two superpowers, it was not the Soviet Union but the United States with which Mao preferred to negotiate. Besides, rapprochement with the Soviet Union could not solve the problem of Taiwan, which—contrary to the views of some prominent American scholars several years ago—had always been the most important issue in Sino-American relations.

It is debatable whether the political differences between Mao and Liu over these two fundamental issues were irreconcilable. Was the Cultural Revolution a power struggle? Snow answered his own question: "Subjective factors cannot be entirely separated from objective reality" (p. 20). Admittedly, the story of the Cultural Revolution is full of important gaps, omissions, and modifying facts. Besides, as Snow stated, we have not heard from the defeated.

Now it was said that Mao and Liu had always represented "two lines" from the beginning of the Chinese Communist movement. Snow's account of Liu seems to be quite fair under the circumstances. Snow remarked that before 1962 Liu had never overtly opposed Mao personally, and that Liu did not seem to have made any planned, serious attempt to meet Mao's challenge in an all-out struggle or even fully realized, before August 1966, that he himself was the No. 1 target of the Cultural Revolution. How could Liu, Snow asked, without specific directives, know that he was supposed to abolish himself and the Party apparatus he headed? He could only act as he always did—use Party cadres to carry out Party policies. Who could have foreseen the Red Guards? All his previous experience with Mao's rectification campaigns had failed to prepare him for anything so drastic as the breakup of the Party machine itself by outside forces. That was where he made mistakes in line and orientation at the beginning of the Cultural Revolution. Perhaps he was no less astonished than the rest of the country at Mao's daring intent.

Mao said that there were two things of which he highly disapproved during the Cultural Revolution. One was lying and the other was the maltreatment of Party members removed from power. But he did not elaborate on either one of these points. The Cultural Revolution was made possible because of the enormous prestige of the Chairman and the support of the People's Liberation Army. Curiously enough, the book contains little information about Lin Piao, and there is a complete blank on the role of Premier Chou En-lai in the various stages of the Cultural Revolution. However, visitors do not ask embarrassing questions. The world was speculating about the fate of Lin Piao after the airplane accident in Mongolia in September 1971. But when I visited China shortly after the incident, his name was not mentioned once by anyone during my entire trip. Apparently, Snow did not ask his hosts the whereabouts of Liu and the other ousted top leaders.

Snow told Mao that he had once attended a conference in which the professors debated whether Mao had or had not made any original contributions to Marxism. He asked one professor at the close of the conference whether it would make any difference in their controversy if it could be shown that Mao himself had never claimed to have made any creative contribution. The professor impatiently replied, "No, indeed. That would be quite beside the point." After hearing this story, Mao laughed and said that more than two thousand years ago a Chinese philosopher had written an essay. A hundred schools of thought then arose to dispute its meaning! Snow must have got some personal satisfaction from this comment.

The Long Revolution contains eight chapters and four appendices. It deals with various subjects, including the commune system, medical care, population control, the Cultural Revolution, the army, and the Sino-American rapproachement. The most valuable parts of *The Long Revolution* are the records of Snow's conversations with Mao and two lengthy dialogues with Chou during Snow's last visit to China, Agusut 1970–February 1971. Some of the material, however, including the discussions of Sino-American relations and the first concrete figures on China's industrial and agricultural output made available in Peking for nearly a decade, had been published previously in newspapers and magazines. Other valuable data in the book include material on the rebuilding of the Party and state superstructure after the Cultural Revolution. For example, the Party membership was suspended during the Cultural Revolution but less than one per cent of the Party membership had actually been expelled. By November 1970, something like 95 per cent of the former Party members had been reinstated. Reinstated did not necessarily mean reassigned; many awaited "liberation" following completion of "struggle-criticism-transformation," the three-stage formula for redemption. As for the fragmentary reports on China contained in the book, the United States has been flooded with similar eyewitness reports since 1971.

When *The Other Side of the River* was published in 1962, it was a major contribution to the understanding of China as Snow found it during his visit in 1960. *Red China Today* is advertised as a revised and updated, enlarged edition of the 1962 book. As a matter of fact, with the exception of the addition of several footnotes, the deletion of several chapters and several very minor revisions, the two editions are identical.

Edgar Snow returned from his last trip to China in February 1971. He died on February 15, 1972, thus ending the unique career of an American journalist who first interviewed Mao in 1936 and brought to the world a sympathetic and influential account of the Chinese Communist movement. He will be remembered by his friends in China.

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Mao's Revolution and the Chinese Political Culture. By Richard H. Solomon. (Berkeley and Los Angeles: University of California Press, 1971. Pp. 604. $16.75, cloth; $5.95, paper.)

This book is the latest in a series of landmark studies of Chinese politics that attempt to integrate the confusing and difficult-to-obtain data emerging from the Mainland with a theoretical orientation drawn from one or another of the social sciences. It is legitimately to be compared with such earlier works as Franz Schurmann's *Ideology and Organization in Communist China,* A. Doak Barnett's *Cadres, Bureaucracy, and Political Power in China,* historical works by Stuart Schram and Jacques Guillermaz, and the Social Science Research Council volumes edited by Barnett, John M. H. Lindbeck, and Robert Scalapino. Like the Schurmann volume, Solomon's work takes a well-developed theoretical approach—his choice is political culture—as the means to order the details of Chinese politics. It is, of course, desirable that scholars continue to try to close the gap, long extant, between the particular study of Chinese politics and differential theoretical and methodological viewpoints on how most accurately and parsimoniously to investigate politics in general. That Solomon's effort is, on balance, less than completely successful, should not be interpreted as signaling the futility of such enterprises, nor does it mean that both political science and Chinese political studies are not better off because of his work.

The work is divided into four parts, the first two of which investigate the details of traditional political culture. The main idea is that the socialization process in traditional and transitional China created a clear pattern of attitudes toward authority—whether familial, political or administrative—that stressed conflict avoidance and accommodation, anxiety when confronted with authority (and hence the avoidance of politics), and the fear of personal betrayal and of social confusion. Thus, concludes the author, Chinese politics and family life is set, at its base, against political change whenever there is the danger of disorder; indeed, against any challenge to authority that involves even the threat of violence. Chinese politics was therefore profoundly conservative: the individual learned to repress deep within his psyche tendencies to right wrongs, to forego any desire to strive for social-economic-political justice, and not to become involved in political movements. It follows that the Chinese historically was much more willing to tolerate political repression and arbitrary government than, say, Western political man, who was subject to an entirely different set of environmental forces.

Having asserted this, Solomon then tries to explain how it is that China underwent a severe revolutionary catharsis, is governed by one of the world's most stringently militant Marxist political parties, and yet does not sink back into the more natural traditional attitudes toward politics. His answer is that a single man, Mao Tse-tung, managed to overcome the socialization process that so molded all other Chinese; and, aided by, learning from, and using several of the developments of Chinese history (warlordism of the 1920s, the Japanese invasion of the 30s, and the increasingly miserable socioeconomic conditions of his compatriots) fashioned a party that gradually led an otherwise unwilling people to overthrow the old system of political culture. The confusion (*luan*) created by these historical circumstances enabled Mao and his associates to enlist large numbers of people in an effort to refashion Chinese society completely, a task made possible by the very fact that the Chinese political psyche, once freed of the limitations imposed by the old political system, tended to go to the opposite extreme and approve of and actively assist in the violent transfer of state power. Thus, the key to the Chinese revolution, as Solomon sees it, is the dialectical relationship between the leader, Mao (who learned how to "turn on" the Chinese people by making essentially the same analysis in the field as Solomon made in the social science laboratory) and all other Chinese (who retained, and still retain, all the nonrevolutionary biases of their traditional political culture).

The last two parts of the book comprise a long historical illustration of this thesis. The history of the Chinese revolution is nothing more than that of the relationship between the single Leader and the masses of the led, inside and outside the Party. The Leader's discovery of the Truth (not of Marxism, but of the nature of Chinese political culture) had set *him* free and *his* life has since been dedicated to informing his brethren of this discovery and then to forcing them to change for the better whether they like it or not. The led have been dragged along, as it were, by the force of the Leader's personality, and show constant signs of falling back (indeed, wishing to fall back) into the rather more comfortable and conventional ways of yesterday.

So the history of the Chinese revolution is the history of Mao's differential success in creating a new political culture to replace the old. That is what the Cultural Revolution was all about, in Solomon's eyes: it was the third of Mao's great attempts to mold the Chinese people in his own image, to kick them into shape (the first two being the long revolutionary road to power culminating in 1949, and the Great Leap Forward in 1957–59). The trouble is that in the long run, the struggle against the traditional culture is an unequal one. Most recent observers wonder if the Cultural Revolution, for all its *sturm und drang,* did anything but confirm that the old ways not only have not been eradicated, but that they may indeed be more powerful than ever. Liu Shao-ch'i, who sought to use traditional Chinese political culture to modernize China, may well yet have the last laugh, and Mao, who still seeks to revolutionize China despite, and to some extent against, the people's wishes, seems to have lost his battle.

This idea adds an important element to our understanding of modern China. The question remains, however, whether it is anything more than heuristic, for there are major methodological problems with Solomon's analysis. For instance, his reasoning is heavily circular in his derivation of the central features of Chinese political culture. First he surveys the prior literature on Chinese characteristics and rejects this as unscientific, then does attitude testing on a group of Chinese in Hong Kong and Taiwan, and concludes that the political attitude structure so revealed must reflect reality since it is congruent with the testimony of the traditional writers. Moreover, his sample is woefully small (less than one hundred to represent the 700–800 million Chinese), male only, and instead of being randomly chosen, is deliberately skewed to reflect regional diversity and uniform age distribution. The statistical variance in interviewer response is extreme. Throughout, there is heavy reliance on the doubtful analogy between some alleged Chinese characteristics, such as emphasis on orality and food, and political characteristics, such as "eating bitterness." More important, it is not clear whether the characteristics elicited are those centrally important to Chinese political culture. In the end, the reader is left asking what Chinese political culture is: how does one know it if it is put before him (i.e., how does one sort out political culture characteristics from the other attributes of the Chinese people?). Indeed, it is not even clear that the author has not induced the heuristic, bare-bones theory out of the facts of Chinese political history (a legitimate enterprise in itself) but then *a posteriori* grafted onto it pseudoscientific (in this case) measurement techniques to serve as weighty "proof." Finally, Solomon fails to explain adequately how it happened that Mao alone escaped the fate of other Chinese people and, operating as much outside and upon Chinese political culture as within it, emerged as a Savior revered by the masses instead of being rejected by them as a crank. Despite the author's good intentions, doubt wells up in the reader's mind, and his natural conclusion is to question whether the political culture approach is the proper shortcut to a sophisticated understanding of latter-day China.

The book falls apart in the second half, as the framework of the first proves not only inadequate but inapplicable. There is just too much that needs explanation, or rather, everything that has happened to the Chinese Communist Party since 1928 and to China as a whole since 1949 is forced within the bounds of the theory. Reality is too complex for the political culture approach (as it is for all other approaches—which is perhaps the way it should be), and the author finds himself casting about for a more viable framework, hopping from political theory to political history to invocation of some of the words of the political culture approach to, in the end, straight Pekinological esoterica and the uncovering of abstruse facts. If the political culturalist feels the necessity to take upon his own shoulders the explanation of everything that happens in a given time-span, and the result is the mish-mash of Parts III and IV, not many will be encouraged to follow in Solomon's footsteps.

The second half of the book can, to be sure, be used as a reference volume on what has happened in China since 1949. Solomon's reconstruction of the Great Leap Forward period, its aftermath, the events leading to the Cultural Revolution, and major aspects of the most recent era of Chinese history, is the first detailed attempt to revise and improve our understanding of post-1949 China, using the Red Guard and other Cultural Revolution documents. It is difficult, however, to plow through 366 pages of facts that do not, despite the author's assertion, serve as illustrative proof of the theoretical conclusions of the first half of the volume, and are not tied together by any other sort of intellectual glue. The book should, indeed, be treated as two separate works. If that had been done in the first place, theory and fact could have been separated, or only relevant facts noted, and the overall evaluation of Solomon's effort would possibly have been much higher. Lest this review end on perhaps too negative a note, I should assert that rather than to advance by slow, timid, and painful increments, it is often better to aim high and fall short. Students of contemporary Chinese politics are indebted to Dr. Solomon for his efforts.

THOMAS W. ROBINSON

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The Managerial Class of India. By M. Subramaniam (New Delhi: All India Management Association, 1971. Pp. 91. Rs. 10.00.)

Social Background of India's Administrators. By M. Subramaniam. (New Delhi: Publications Division, Ministry of Information & Broadcasting, Government of India, 1971. Pp. 180. Rs. 12.00; £1.25; $3.60.)

The author of these monographs on the social composition of India's governmental and managerial elites brings impressive qualifications to his task. Professor Subramaniam, trained in public administration in India and Australia, has conducted several similar studies in both countries and was twice prize-winner in the Haldane Essay Competition of the Royal Institute of Public Administration. The first of the books here reviewed results from a study done while teaching recruits to the higher civil service at the National Academy of Administration. The author thus had access to a large body of official data about his subjects as well as personal contact with many of them. While still at the National Academy, Professor Subramaniam felt that studies of other middle-class groups should be undertaken for comparative purposes. *The Managerial Class of India,* encouraged and sponsored by the All India Management Association, results from a project in which managers of five "highly management-conscious firms" were compared with those of firms drawn as a random sample from a list of 200 top firms published by the Federation of Indian Chambers of Commerce and Industry.

In both studies a large mass of data was assembled from application and recommendation forms, questionnaires, and interviews. These were coded and processed by sorter and computer; tables were then constructed to show the interrelation of social characteristics in the populations concerned. In the volume on civil servants the subjects are mainly direct recruits through competitive examinations, from 1947 to 1963, of the Indian Foreign Service, Indian Police Service, and Indian Administrative Services. In the first two groups the total populations (144 and 772, respectively) were studied; in the last-mentioned, 45 per cent of the total. The rationale for emphasizing direct recruits is that they comprise the majority of the foreign and central services, and there is generally a trend toward direct recruitment by examination and progressive retirement of mature-age recruits in the Indian civil services. In the case of the central services—departments administering matters under the federal government, as distinct from the Indian Administrative Service which administers matters under state jurisdiction—a different method of selecting the study population was employed: the total numbers of the two more prestigeful and the two less prestigeful services were surveyed. In the book on managers, more than 1500 individuals supplied data via detailed questionnaires circulated by the chief personnel officer of each firm. While there was inevitably some variation in various firms' definition of the term "manager" and in percentage of response, these factors do not seem to be of sufficient magnitude to call into question the conclusions drawn.

In the first book the author addresses himself to the question of the representativeness of Indian bureaucracy. To the surprise of no one at all familiar with Indian society, bureaucracy turns out to be very unrepresentative indeed. How important is this? Professor Subramaniam argues that representativeness per se is less important than responsiveness to public needs, and that the latter is influenced, at least in other countries, by sharing of "common values which are part of the nation's socio-political consensus" and the exposure of the bureaucracy "to all influences regardless of their class origins" (*Social Background of India's Administrators,* p. 6). He also stresses the importance, in a developing society, of efficiency, and contends that this is best achieved by rigorous selection through tests of ability:

> For India clearly, the rigorous selection of an efficient bureaucracy should be placed far above a concern with its *immediate* representativeness. That will come about gradually and faster than suggested by hasty critics through the widening of educational opportunities for all and the provision of intensive education for the under-represented. Meanwhile, suitable training methods may be tried to make it more responsive (*Ibid.,* pp. 6–7).

The unrepresentativeness of public bureaucracy in India is a function to a great extent of the very small middle class—only 10 per cent of the population, as compared to 60 per cent in the U.S. The middle class dominates the civil service in both countries; in India its degree of over-representation in the civil service is more than 9 times its population percentage; in the U.S., only 1.5. The author suggests, for India, that "the natural growth of the middle class in the wake of economic development is bound to correct its over-representation" (*Ibid.*, p. 128). He rejects reforms which would immediately establish proportionate representation for all classes on the ground that the very low educational standards it would entail would be seriously damaging. Prof. Subramaniam thinks that the inevitably slow process of widening educational opportunities up to university level, which would first help the lower middle class to qualify but would be rather unlikely to greatly assist the agricultural and working classes, would be of some limited value. He raises the question whether, through sophisticated research, it may be possible to deepen the understanding and sympathies of civil servants for other classes—which, in his view, already exists to an extent greater than is commonly assumed. What is implied is some sort of in-service political socialization to counteract the tendency for middle-class bureaucrats to harden in their class-based attitudes and to emulate those of their superiors as their careers lengthen.

The title of the concluding chapter of *Social Background of India's Administrators,* "A Middle Class Monopoly—So What?," is an apt one which accords well with the author's closing remarks: "We may not have the best or the most representative civil service in the world, but any drastic change in the modes of recruitment would produce more harm than good. There is much to be said for a policy of controlled experiment, careful research and gradual change" (pp. 130–131). Ideally, this may be so; yet this reviewer cannot but wonder, in view of clear signs of serious economic deterioration in India within the last year or so and the impact of this decline upon the outlooks of all classes, the bureaucracy included, whether time is on the side of gradual improvement.

The findings of *The Managerial Class in India* show important similarities and differences only of degree:

> . . . both higher civil servants and managers are drawn more heavily from the coastal Presidencies and the corresponding linguistic groups than from the heartland of India. . . . Both include a large proportion of mobile inter-state affiliates the proportion being higher for managers. Both are drawn heavily from the middle class but over the last decade the lower middle class component has increased steadily in the civil services, partly naturally and partly through governmental policy, while there is no similar tendency among the managers. . . . The main point of difference between managers and higher civil servants seems to be the higher proportion of science and engineering graduates among the former (*Ibid.*, p. 36).

Indian managers show a high degree of professionalization; for this fact the author tries to sketch an historical explanation in a purely hypothetical way, since no definitive history of Indian business and commercial development exists to provide adequate data.

Both of these books are extremely useful in providing a clear and empirically adequate sociological profile of two important segments of India's political and economic structure. If they do not offer any ready solutions to problems, at least they enable the interested reader to perceive the problems in sharper outline.

CHARLES S. BRANT
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Catholicism and Political Development in Latin America. By Frederick C. Turner. (Chapel Hill, N.C.: University of North Carolina Press, 1971. Pp. 272. $8.75.)

While this book occasionally dips into the past, and more frequently looks outside Latin America to Europe for the philosophical roots of the positions it describes, Turner's is essentially a snapshot. It portrays the highly divergent views held today by Latin American Catholics, clerical and lay, as they face issues critical to the continent as a whole and to themselves as Catholics and to the Catholic Church in particular.

Turner has worked immensely hard and systematically going through a large and scattered literature, published and unpublished, as well as conducting scores of interviews. More important, he describes all points of view sensitively and, on the whole, fairly as he reworks his raw data. The "left," "center," and "right" within the Catholic world begin with very different assumptions about the nature of social change and development, and about the proper place of the Church and religion in man's life. Turner brings out these differences in basic assumptions before describing the differences over specific issues to which they lead: whether the church should or should not support radical reforms; the precise role it should play even if it does in principle support them; the stand it should take *vis-à-vis* non-Catholic radicalism, violence, and Marxism; the solutions advocated for the manifold problems internal to Catholicism itself; its views on the population problem; attitudes toward the U.S. and so forth.

The author appears to me to be fair to all camps even though intellectually, and to some extent even emotionally, he seems to be somewhat more allergic to the extreme left than to the right. Indeed, he is not very sympathetic even to those "next" to the extremists (persons such as Ivan Illich and Dom Helder Camera). He clearly finds it easier to see saving graces in the passionate anti-communism of Archbishop Rossell Arellano of Guatemala than in the idealism of Camilo Torres, which ended so tragically. The book ends with a breath-takingly explicit plea that the U.S. government support moderate progressives such as Chile's Christian Democrats with all the means at its disposal—but perhaps through multinational agencies in order not to compromise the receiver! I doubt that policy recommendations of this kind are an appropriate end to an otherwise analytical piece of work.

Nevertheless, and paradoxical though it may sound, this is essentially a fair, well-researched, and sensitive portrayal, whose major blemish lies in the rather loose organization of its six chapters and some unnecessary repetition of material. The chapter on "Catholic Revolutionaries," for example, deals at much greater length than the title would suggest with the causes of the increasing popularity of evangelical sects in Latin America, and the reaction of all parts of the Church to it. The same chapter also goes at length into the growing shortage of priests: like other problems, a worthwhile one to discuss, but not while evaluating the position of Catholic revolutionaries. Similarly, in the chapter on "Tradition and Reaction," Turner deals at great length with the relationship of the Church to Peron: a story that deserves to be told but which does not bear a relationship to the title of the chapter except at one or two points. Thus, several chapters lose focus either through tackling problems which are of importance, but fall outside their scope; or by presenting historical accounts of important events, only some of which relate to the main theme of the chapter.

But perhaps the ferment and partial confusion of these chapters reflect their subjects more accurately than would an aseptic and overorganized analysis. For the reality described in the book is determined, at least in part, by certain key figures whose unique styles, histories and positions have had much to do with the particular impact of each. And even apart from any "great man" interpretation of events such as those in Guatemala, the position of no group is sufficiently coherent to justify, or possibly even enable the drawing up of, a schematic outline. This opaqueness is one very important characteristic of the position of Latin American catholicism (of whatever orientation), and the author brings it out, both intentionally and unintentionally, through his failure to organize his material sufficiently. Indeed, apart from confusion, there is one more characteristic that might have been highlighted: the existence of many a void. Many laymen, priests and even prelates continue to take no specific position on various critical issues because they are unaware of them as being critical, or are unable to integrate them into traditional modes of thought.

HENRY A. LANDSBERGER
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Essence of Decision: Explaining the Cuban Missile Crisis. By Graham T. Allison. (Boston: Little, Brown and Company, 1971. Pp. 338. $4.50 paper.)

This study was summarized at the American Political Science Convention in September 1968, and in the lead article of this *Review* in September 1969 under the title of "Conceptual Models and the Cuban Missile Crisis." In brief, the book sets forth three decision-making models which are applied to the Cuban Missile Crisis: Model I, The Rational Actor; Model II, The Organizational Process; and Model III, Governmental Politics. The three analytic chapters on these models are interspersed with three substantive chapters on the details of the crisis itself, and may be read separately for the sake of historical continuity. Since the author states that all the material used in this study is in the public domain, and since there are several different interpretations of the crisis itself (A. Horelick and M. Rush, *Strategic Power and Soviet Foreign Policy,* 1965; E. Abel, *The Missile Crisis,* 1966; R. F. Kennedy, *Thirteen Days,* 1969; N. S. Khrushchev, *Khrushchev Remembers,* 1970, *inter alia*), the focus of this review will be primarily on the three conceptual models.

Some years ago, Richard A. Brody defended his model of "international reaction" (O. R. Holsti, R. A. Brody, and R. C. North, "Measuring Affect and Action in the International Reaction Models: Empirical Materials from the 1962 Cuban Crisis," *Journal of Peace Research,* Vol. I, [1964]) in a rather disarming fashion. He stated in effect that "while modeling has no particular virtue, it also has no particular vice." The main working hypothesis of Graham Allison's study might be stated; "That while modeling does have some particular virtues, it also has some particular vices." That is, the main proposition set forth in Allison's study is that; "Professional analysts of foreign affairs (as well as ordinary laymen) think about problems of foreign and military policy in terms of largely implicit conceptual models that have significant consequences for the content of their thought" (p. 4). Allison sets for himself the task of making these models *explicit* and then delineates two subordinate propositions which state the intellectual framework of the study: "Most analysts explain (and predict) the behavior of national governments in terms of one conceptual model here entitled Rational Actor or 'Classical' Model . . . ," and "Two alternative conceptual models, here labeled an Organizational Process Model . . . and a Governmental (Bureaucratic) Politics Model . . . , provide a base for improved explanations and predictions" (pp. 4–5). As working hypotheses, these two subordinate propositions form the backbone of the study.

In treating the Rational Actor Model, the study points up the fundamental intellectual, psychological or analytical weakness upon which the model is premised: rational political man. However, it is doubtful if any of the foreign policy analysts who use this general model, do so on the assumption of rational man and/or rational behavior, since neither rationality nor rational behavior can be proved *a priori.* The determination of whether or not a decision/policy/program is rational is largely made *ex post facto* in relation to the objective(s) it was supposed to achieve or the goal(s) it was supposed to try and fulfill. That is, if the decision produced an outcome that was satisfactory or successful in terms of the objective(s)/goal(s) it was supposed to achieve, then it might be considered "rational." For example, if the primary objective of the Kennedy Administration in the Cuban crisis was to get the "offensive weapons" out of Cuba, then the decision/policy/program arrived at was "rational" since the desired result was achieved, although it may not have been done in an optimal manner. However, even such a definition of rationality in terms of some previously stated objective is insufficient and inadequate unless it is placed within the larger national and international context and unless the costs involved in achieving the objective are carefully assessed. That is, if the Khrushchev Administration had not agreed to withdraw the offensive weapons on Sunday, October 28, and if the Kennedy Administration had felt compelled to fulfill its' "explicit threat of air strike or invasion on Tuesday," October 30 (pp. 65 and 247—a new and possibly contentious point), then it might well be asked whether the decision/policy/program was rational in terms of the costs involved. On moral grounds, it is interesting to note that Robert Kennedy had intended to write about the "basic ethical question involved: what, if any, circumstance or justification gives this government or any government the moral right to bring its people and possibly all people under the shadow of nuclear destruction?" (*Thirteen Days*, p. 128).

The greatest value of the Allison study seems to be in the explication and development of two alternate models. The substance and style of the study in dealing with these two models is far more detailed, thorough and convincing than is the treatment of the first model. These chapters raise these two approaches or models to a new level of sophistication and analysis applicable to questions of foreign policy and national security. The first of these two models is what Allison terms the "Organizational Process" model. It draws heavily upon Herbert Simon's *Administrative Behavior* (1957), James March and Simon's *Organizations* (1958) and March's *Handbook of Organizations* (1965). The need for such a model is quite clear, because as Allison notes: "Few specialists in international politics have studied organization theory. It is only recently that organization theorists have come to study organizations as decision-makers; behavioral studies of foreign policy organizations from the decision-making perspective have not yet been produced" (p. 69). While this observation may not be entirely accurate (Henry A. Kissinger, *American Foreign Policy*, 1969, I. M. Destler, *Presidents, Bureaucrats and Foreign Policy*, 1972, and David H. Davis, *How the Bureaucracy Makes Foreign Policy*, 1972), the point is well taken. The second of these models deals with "Governmental Politics" and draws heavily on Richard Neustadt's *Presidential Power* (1960), Roger Hilsman's *To Move a Nation* (1967) and Charles E. Lindblom's *The Policy Making Process* (1968). It emphasizes the importance of obtaining internal bureaucratic consensus on ends and means in order to achieve the desired outcomes. Almost inevitably, so far reaching a consensus has been difficult because, as the author notes, attributing the phrase to Don K. Price: "Where you stand depends on where you sit" (p. 176). The inferences and implications of "Price's Law" for decision makers are most interesting, whether they are defined as the President, Chiefs, Staffers, or Indians.

Allison concludes that each conceptual model greatly influences the assumptions, the tools of analysis and the results produced. This seems only logical since different conceptual frameworks elucidate particular aspects of a question, problem or issue, which can then be synthesized to explain so complex a phenomenon as the Cuban crisis. In the field of international politics/foreign policy, there are easily two dozen or more identifiable analytic approaches (e.g., James Rosenau, *International Politics and Foreign Policy*, 1969; James E. Dougherty and Robert L. Pfaltzgraff, *Contending Theories of International Relations*, 1971), upon which the researcher can draw to help analyze, evaluate, and interpret the various phenomena of international relations, crisis management, and conflict resolution. If the same analytic rigor exercised by Allison were also utilized in applying some of these other approaches to the Cuban crisis and other international phenomena, we would add greatly to our insight into such phenomena and to our understanding of the validity of the various methodologies used.

DAVID LLOYD LARSON
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China and Japan at War, 1937–1945: The Politics of Collaboration. By John Hunter Boyle. (Stanford: Stanford University Press, 1972. Pp. ix, 430. $16.50.)

John Hunter Boyle has written a fascinating account of the efforts of a few Japanese and Chinese to substitute collaboration for war to enable their peoples to stand together against communism and Western imperialism. His book is easily the best study of the Chinese interest in collaboration and of the thought and operational problems of the Japanese who reached out to them. Based on an impressive array of Japanese and to a lesser extent Chinese sources—and interviews with participants—Boyle's analysis unravels a host of tangles, especially of those threads leading from Chinese "puppets" back to Chiang Kai-shek and from Japanese negotiators to their nominal superiors in Tokyo. His conclusions are not likely to be modified—unless publication of his work jars loose new documentation from Chiang's personal files.

After considering the general problems of studying so-called puppet governments, Professor Boyle focuses on the Wang Ching-wei regime. He examines Wang's relationship to Chiang Kai-shek, and presents overwhelming evidence that Chiang knew and approved of Wang's initial efforts toward peace with Japan. He also explores Wang's personal motives for negotiating with and eventually coming to terms with the Japanese and does justice to the complex mix of patriotism, ideology, and ambition involved.

More intriguing is the story of the Japanese negotiators from whose perspective Boyle can see most clearly. Central to their efforts was the pan-Asian thought of Ishiwara Kanji, the brilliant militarist forced into retirement because of his opposition to the Army's—and the Japanese government's—imperialism in China. Although himself an architect of Japan's seizure of Manchuria, Ishiwara came to believe that the evolution of Chinese nationalism, combined with the growing threat of Soviet communism, required Japan to go off on another tack. He insisted that Japan be generous with China, respect China's national aspirations, work shoulder to shoulder with China against the Soviet Union—rather than drive China into alliance with the Russians and be faced with a two-front war on the Asian mainland. The men who negotiated with Wang Ching-wei shared Ishiwara's vision. They understood and respected Chinese nationalism and sought to work with Wang not because they wanted a puppet but because they hoped to build a bridge to the Chinese Nationalists. But the efforts of these men, of Kagesa Sadaaki, Inukai Ken, and Imai Takeo, failed to produce the generous settlement all involved believed necessary because the Japanese negotiators could not obtain support for their program from the elements dominant in the Japanese Army or from the prime minister.

Once again we are granted insight into the incredible divisions that existed within the Japanese government, the ability of various commanders and their units to act independently of civilian or military authority in Tokyo. In China, army officers with their own ideas of what Japanese policy in China should be created their own puppet regimes, ignoring orders to the contrary. A host of Japanese agencies functioned in China, each intent on creating its own empire. For the most part there was only one shared element in Japanese planning and operations in China: an incredible ignorance of Chinese nationalism and a refusal to heed those few Japanese, like Ishiwara and Ishii Itaro, who understood that China in the 1930s was not the old China of easily manipulated warlords. Such ignorance of another people's society, such operational independence on the part of military commanders would have been inconceivable to Americans—before Vietnam.

The interpretation of the New Order for East Asia that prevailed in the Japanese Army and was shared by Konoe left little leeway for Chinese who wanted to work with Japan. North China would have to be separated from any "national" Chinese government—the Army in North China would not surrender its fief. Inner Mongolia and Manchuria would cease to be Chinese territory. China's resources were to be "shared" with Japan on an obviously exploitative basis. On all these questions the Army commanders and agency heads involved were adamant, unwilling to consider more generous terms for peace even as the tide of war shifted in 1944. Boyle's analysis of the concessions Konoe could offer might be useful to students of Japanese-American relations who have wondered if the United States missed an opportunity to reach an accommodation with Japan in 1941.

Perhaps the most important of the many insights granted by a reading of this volume comes from the evidence that Japanese policy toward China and the rest of the world did not result from a rational assessment of the national interest conducted by the prime minister or responsible officials. On the contrary it is obvious that "Japanese policy" was in reality an aggregate of decisions made by various officers in the field and by various agencies at home over which the center, whether civilian or military, exercised little influence. This pattern of policy making will surprise few political scientists (and perhaps not many historians), and it is disappointing, therefore, to have Boyle nonetheless make frequent reference to "Japanese policy" or "Japan's purpose" as if the Japanese decision-making body was a monolithic force. Despite this caveat, the book is a most valuable contribution to the understanding of Japanese thought and policy during the 1930s and 1940s—and to the history of China and Japan as well.

WARREN I. COHEN
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World Without Borders. By Lester R. Brown. (New York: Random House, 1972. Pp. 395. $8.95.)

Provocative and readable, this book would be a good addition to the reading list of any introductory course in International Relations. Mr. Brown's analysis ranges over a wide variety of issues: environmental crisis, rich-poor gap, unemployment, urbanization, hunger, education, population, East-West relationships, economic interdependence, communication, transportation, multinational corporations, supranational institutions, and the quality of life.

The book's extensive scope contributes, of course, to some degree of superficiality. And there are occasionally irritating repetitions. But the work's primary value is its dynamic treatment of current and projected trends in international affairs for general readers—and especially for undergraduate students.

One should judge *World Without Borders* in the context of the present transitional period in American foreign policy. The book certainly illumines the extent of foreign policy choices and calls into question the nature of American involvement in international affairs.

The two central issues considered in *World Without Borders* are social justice and the quality of life. Social justice is a perplexing problem for Americans, I would suggest, since it has been only a peripheral topic in our political history. Brown—a Senior Fellow with the Overseas Development Council—does a good job of conceptualizing the rich-poor, or North-South, gap in the world, but he doesn't really demonstrate his contentions about the "great stress on the international political fabric" which the growing economic disparities will cause. As Brown argues, technologically advanced societies are highly vulnerable to sabotage and disruption of energy supplies and raw materials. But social and political unrest in the poor nations will damage their own future more than it will harm the living standards of the rich countries.

Distributive justice requires more than humanitarian sympathies or assertions about future North-South conflicts. It necessitates a higher level of political leadership within the technologically advanced societies. One approach, recently suggested by Frederick Nossal, is an international development tax. In a sense, there are already international taxes—paid by members of the World Bank and the United Nations. Perhaps the United States and other industrialized nations could return to the "luxury tax" system to finance global development programs.

Finally, Brown discusses the concept of the quality of life—which has become a fashionable political issue in recent years. "Reduction in poverty on a global scale," he says, "can be viewed either as a narrowing of the gap between rich and poor nations, or in terms of providing a minimum level of living for all people. Over the shorter term, the latter is a more realistic goal both because it is more easily attainable and because it focuses more directly on the critical human needs" (p. 322). In essence, the author urges a global minimum living standard—expressed in social rather than economic terms.

Regarding the rich nations, Brown argues that per capita income is becoming less satisfactory as an indicator of well-being, since the decline per person of natural amenities tends to offset growth in income. In America, the quality of life is a potential political bombshell; only the preliminary aspects of this political problem have appeared up to now.

Although some political scientists will be disappointed with the political analysis in *World Without Borders,* it is an impressive treatment of the threats to the well-being of mankind.

ROBERT H. PUCKETT
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The UN and the Palestine Refugees: A Study in Nonterritorial Administration. By Edward H. Buehrig. (Bloomington, Ind.: Indiana University Press, 1971. Pp. xvi, 215. $8.50.)

In view of the mountains of words that have accumulated on the many aspects of the "Palestine Question," as it originally was, it is curious that Professor Buehrig's should be the first full-length study of UNRWA (UN Relief and Works Agency for Palestine Refugees in the Near East). For one thing, it is an institution quite different in several respects even from other operating welfare agencies of the UN system constituted under Article 22 of the Charter as subsidiary organs of the General Assembly—for examples, UNHCR (High Commissioner for Refugees) and UNICEF (the Children's Fund). This alone would make it worth the primarily legal and administrative study that Buehrig has written. One reason such a study has not appeared before now is because it could hardly have been written from published records alone; the author did considerable fieldwork and had the cooperation of officials experienced in the actual operations of the Agency to help him put flesh on the bare bones of formal documentation.

In addition, UNRWA is one of the oldest continuing UN operational bodies, which justified an historical review after nearly a quarter-century. The study is a useful reminder of how and why it came into existence and what changes it has undergone in that period. This is all the more important since UNRWA is an embarrassing reminder of a basic element in the broader problems of Arab-Israeli relations and those of the major powers in the region—an element that governments and people are prone to push under the rug of more urgent political controversies, while withholding essential resources if the Agency is to carry out the responsibilities so regularly given it by a distant and often indifferent Assembly.

The author emphasizes the political as well as humanitarian origins of UNRWA and frequently notes underlying political causes of the legal and administrative difficulties discussed. The most basic of these was the mandate in Paragraph 11 of the 1948 Assembly Resolution 194(III), which authorized repatriation of those refugees who desired it and compensation for those who chose resettlement. This provision conflicted with the perceived requirements for a viable Jewish state; and of course there has yet to be a reconciliation through some compromise peace settlement to replace the 1949 Armistice Settlements. Buehrig ends his study with a wholly political conclusion that reverts to the origin of the problem:

> As it did in the partition resolution of November 1947, the General Assembly could once again declare its support of a Palestinian Arab State. In such a context, the vain promise of repatriation could be abandoned. Furthermore, UNRWA would gain a new rationale. Instead of the invidious task of seeking through individual welfare to mollify a people for loss of a homeland, the Agency's acknowledged role would be to sustain the emergence of the Palestinians as an independent national community. . . . The United Nations could then relate to the Palestinians—for whose plight it cannot escape a measure of responsibility—as a source of financial and technical assistance. While the many repercussions of such a move by the General Assembly are not easily predictable, the net result might be better rather than worse (p. 189).

Although it is unfair to criticize an author for not writing a book he never undertook, it must be said that it is also unfair to politically minded readers to put forth such a proposal without any discussion of its profound political implications, not even of those "repercussions" that might be fairly "easily predictable." This is especially so since, given the political content already included in the study, it is difficult to see how an Assembly declaration in the 1970s could be much more effective than the earlier proposal for the partition of Palestine.

Meanwhile, we can be grateful for this valuable analysis of the constitutional origins, the legal and operational problems of this unique agency. UNRWA was established by the General Assembly as a result of the unexpected turn of events in 1948 after Britain terminated its mandate over Palestine. It was given two objectives: the urgent one of immediate relief for hundreds of thousands of Arab refugees displaced by the war; and the longer-term one of creating employment as a means of assisting permanent resettlement of the refugees in neighboring countries. Buehrig describes how its activities in the first field evolved gradually into its current programs of health and welfare, while it was completely frustrated in its latter objective. He analyzes its constant struggle for autonomy and the problems of control growing out of its three-way relationship with its parent Organization, its host countries, and its contributing governments. Its mandated economic approach, in short, failed to resolve a problem bitterly political both in origin and in its continuing character.

After 1965, as Buehrig shows, UNRWA consequently turned from the "attempt to integrate displaced Palestinians into the surrounding Arabic society through economic programs," to the "cultivation of individual capabilities through basic education and vocational, technical, and professional training" (p. 147). One of his most interesting themes is that this gradual shift in emphasis eventually resulted in unexpectedly fostering the nationalist movement among the Palestinians, who were originally a politically and socially fragmented mass. UNRWA now, he concludes, "raises the question of whether humanitarian regard for the needs of refugees has been changed into subsidy of a political movement" (p. 189).

The answers to this and other questions raised by the UNRWA experience are not as simple as the political polemics on both sides would have us believe; but this study assists in clarifying them in many ways.

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Gehlen, Spy of the Century. By E. H. Cookridge. (New York: Random House, 1972. Pp. 402. $10.00.)

The Game of the Foxes: The Untold Story of German Espionage in the United States and Great Britain During World War II. By Ladislas Farago. (New York: David McKay, 1972. Pp. 696. $11.95.)

Project Paperclip: German Scientists and the Cold War. By Clarence G. Lasby. (New York: Atheneum, 1971. Pp. 338. $8.95.)

The Double Cross System in the War of 1939 to 1945. By J. C. Masterman. (New Haven: Yale University Press, 1972. Pp. 203. $6.95.)

OSS: The Secret History of America's First Central Intelligence Agency. By R. Harris Smith. (Berkeley: University of California Press, 1972. Pp. 458. $10.95.)

No more fearsome research frontier exists than the secret and covert foreign operations of governments in war and peace. Fearsome, because hard evidence is elusive. The subject is important because enormous resources in war and peace have been allocated to secret warfare and to ferreting out secret information; and information is a crucial variable in decision making. During World War II thousands of individuals labored in the nether world of "strategic services." When the Truman doctrine was proclaimed in 1947, permanent secret strategic service and intelligence agencies were determined as essential to United States security. Since then, tens of thousands of persons have been engaged in a variety of secret activities costing billions of dollars. Most of this activity has occurred beyond public scrutiny. Indeed much of it has gone forward beyond the span of control of executive, let alone legislative authority.

This raises questions: What has been the nature and consequence of these secret governmental activities? What has been the basic setting for policy, organization, and responsible control? What have been and are the costs and the benefits? Recent books, listed above, shed some backlight on these questions.

J. C. Masterman's *The Double Cross System* is the most significant and unique of these works. Significant because it deals authoritatively with an important secret operation during World War II; and unique because it is a genuine secret document. Sir John, a British historian and Oxford don, wrote it in the summer of 1945 as a top secret report. It is an account of how British Intelligence, through an operation called "Double Cross," gained control of the entire German network of spies in Great Britain. The German agents were "turned around" by their British captors and used for counter-espionage and to deceive their former bosses. Deception about the time and location of the Normandy invasion in June 1944 was probably essential to success on D-Day.

Publication was ultimately urged upon the British government by the author because of its potential propaganda value. Many had come to doubt the efficiency of the Secret Services or its efficacy as a foreign policy instrument. Masterman became disturbed by such low public esteem and determined to gain governmental permission to publish because, as he writes elsewhere, "might not the relating of past successes do something to maintain belief in the efficiency of the service?"

But the government insisted upon censoring certain passages before allowing publication. This raises the question of whether the book, given an indeterminate amount of censoring, and given Masterman's primary motive in his ten-year struggle to get permission to publish, is not in some ways self-serving. Whether it is self-serving or not, it does paint the picture of a remarkable success. Masterman concludes by finding that "espionage in an enemy country is doomed to failure because the dice are hopelessly loaded against the spy" (p. 187). Furthermore, "espionage in wartime is difficult and usually unprofitable; counter-espionage is comparatively easy and may yield satisfactory results" (p. 188). He argues, however, that peacetime espionage is "easy and profitable." These are moot points. This work will be of use not only to espionage buffs, but to students of bureaucratic and international politics.

To *OSS,* R. Harris Smith has assigned an extravagant subtitle, *The Secret History of America's First Central Intelligence Agency.* Mr. Smith is a political scientist who served briefly in the Central Intelligence Agency. His book does not live up to its subtitle's promise. On a subject that has evaded most serious writers, Mr. Smith has produced a work that adds to our knowledge about a major World War II enterprise: the Office of Strategic Services. Denied OSS archives that remain padlocked under the custody of the OSS offspring, the C.I.A., Mr. Smith's "secrets" come primarily from contacts with 176 alumni of OSS. Even if the reliability of their memories after a quarter of a century is questionable, the author has tapped a valuable historical resource. Relying on their memories after 25 years, and on a thorough search of thin bibliographical resources, he recreates some major OSS episodes in major war theaters. These range from the complex maneuvers in North Africa after the fall of France in 1940 to the tragicomic and confused American activities in Indo-China towards the war's end. Smith concludes by comparing the OSS with its successor agency, the CIA, which he calls "the mirror image of OSS." The author gives too much uncritical credit to OSS, without ever making sufficiently clear what were its contributions to victory. His characterization of CIA as a "liberal" force since World War II is inadequately developed. What we have, in effect, is some additional, maybe important pieces of a jigsaw puzzle, but the whole picture remains unassembled.

Project Paperclip: German Scientists and the Cold War, by historian Clarence G. Lasby, is an account of a little-known American operation after World War II in which the United States government "imported" more than 600 alien scientists and technologists for research and development in the atmosphere of Cold-War competition with the Russians. When the Third Reich collapsed, French, British, Russian and American intelligence teams organized competitive secret programs to recruit German scientists with special skills, particularly in military weaponry, especially rocketry. Most branches of the U.S. government, civilian and military, were involved. These bizarre programs came to be assigned the code name "Project Paperclip." Few Americans were aware of the coordinated, deliberate effort to compete for this Cold War human booty—what someone has called "intellectual reparations." Professor Lasby, of the University of Texas, has carefully interviewed more than 200 participants. Unlike Smith, he had access to government documents never before exploited. He sheds light on organization behavior as well as international politics, particularly Cold War origins. This is a model study of its sort, written with clarity and economy, and telling an unknown story.

E. H. Cookridge is a popular writer on espionage. His *Gehlen, Spy of the Century* is one of several recent books dealing with the uncomfortable fact that the United States relied heavily for many crucial years during the Cold War on the West German intelligence system organized and led by one of Hitler's chief intelligence leaders. Until recently, little was known about this invisible but powerful figure (who was Hitler's man for espionage and counterespionage in the Soviet Union). At war's end, General Reinhard Gehlen was hired and subsidized first by the U. S. Army and then by the C.I.A. with hundreds of millions of dollars. In 1956, the Gehlen organization became the West German *Federal Intelligence Service* (B.N.D.). In this book we learn that the American intelligence establishment, without hesitation, was willing to support lavishly Herr Gehlen, who, according to Cookridge, "served Hitler with unswerving loyalty until the bitter end . . ." (p. 7).

The book displays the apparatus of scholarship, but it is surprisingly uncritical. Gehlen is portrayed as something of a hero but such a view probably will not survive a more critical examination of his overall role in exacerbating the Cold War. Perhaps Gehlen's most significant input in this regard was that he supplied seventy per cent of NATO's intelligence on Soviet and Eastern European affairs, according to Cookridge. This book has value in what it reveals, more implicitly than systematically, about intelligence policy, organization, doctrines, and use. But the reader is left to make his own judgment of the efficacy of Gehlen's massive apparatus.

A similar deficiency characterizes Ladislas Farago's *Game of the Foxes*. Farago, another writer of popular works, has produced a mammoth spy "bedside reader," of uneven historical value. Its principal claim to originality is its foundation on German secret intelligence archives, hitherto unused. These Farago has supplemented by dint of a prodigious amount of legwork, with interviews and a variety of unexploited archives. He has produced fifty-three vignettes; some of them have little to do with the *Abwehr*, and no analytical structure supports them. What did all of this secret activity add up to? In a few introductory paragraphs the author hints that it added up to very little, and he hints that it should persuade us to put little reliance on secret services. Again, the reader is left to make his own interpretation.

In surveying my five-foot-shelf of books on this subject, I accept these particular works as additional pieces of an important jugsaw puzzle —a puzzle still far from being assembled. These books contain hints at answers, albeit incomplete, to the questions posed initially in this review. Organizational behavior of secret intelligence organizations appears to transcend national boundaries; their behavior is shaped in the context of bureaucratic politics; much of their activity is useless; and yet evaluated information ("intelligence") is sometimes a pivotal element in decision making.

The term "strategic intelligence," however, remains a compound of definitional and analytic confusion. It is used loosely to cover a variety of disparate meanings and functions, ranging from information gathering and analysis to espionage, sabotage, covert and clandestine political action, and to counterintelligence and paramilitary operations. These books provide only raw material for a better understanding of this secret side of government. They make little contribution to definitional or conceptual clarity on the subject.

Harry Howe Ransom

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The French Presence in Black Africa. By Edward M. Corbett. (Washington, D.C.: Black Orpheus Press, 1972. Pp. xi, 209. $12.50.)

At a time when French-speaking Africans are getting less and less Francophile, and when French aid to Africa is drawing increasing fire from metropolitan observers and African recipients, Professor Corbett's book is both timely and instructive. Rarely has French dominance in Black Africa been subjected to more intense scrutiny—and more devastating criticisms. More concisely and systematically than anyone else so far the author shows how little impact the formal granting of independence to France's African colonies has had on their subsequent relationships with the former imperial power; how, through various bilateral aid agreements, the recipients have placed themselves within the magnetic field of French military, political, economic and cultural influences; and how, through what he terms "the French mind-set of the African political and military establishments" (p. 1), these establishments have unwittingly facilitated France's cultural pre-eminence in the area.

In his earlier and excellent work on French Canadian nationalism (*Quebec Confronts Canada*, Johns Hopkins Press, 1967) the author dealt extensively with the plight of the Francophone minority in the face of a dominant "alien" culture. The shoe is now on the other foot. After pleading the cause of *Francophonie* in North America, the author takes up the cudgels against its European advocates and "native" hirelings in Black Africa. Ultimately, nearly every facet of the French presence in Africa is brought under the microscope and critically examined.

The chief merit of Corbett's *The French Presence in Black Africa* lies neither in the originality of his thesis nor in the nature of the evidence brought to bear upon his analysis. In one form or another the "French connection" to Black Africa has been a central theme in the works of Samir Amin, Theresa Hayter, and Tibor Mende. Indeed, for anyone acquainted with Mende's most recent work (*De l'Aide à la Recolonisation: Les Leçons d'un Echec* [Paris, 1972]), the volume under review has relatively little to offer substantively. The value of the book lies elsewhere. Besides making available a body of data which is often too dispersed (or as yet untranslated) to be readily accessible, the theme around which it is constructed is one which has not received as much attention as it deserves from English-speaking social scientists. By drawing attention to the external roots of the dependency relationship which currently ties much of French-speaking Africa to metropolitan France, Corbett suggests some new and significant perspectives from which to look at problems of change and development.

Unfortunately, the author does little more than hint at some of the dimensions of analysis which need to be explored to do full justice to his topic. While there is some advantage in dealing serially with the various facets of the French presence in Black Africa (political, military, economic, cultural, etc.) one often wonders how exactly each relates to the others. What, for example, are the links between, on the one hand, French expatriate enterprises and aid agencies and on the other, the more explicitly political aspects of the French presence? What are the relationships between metropolitan pressure groups and the various aid agencies involved in *Cooperation?* What is the nature of the connection between the loosely-knit, camarilla-like organization built around Jacques Foccart (officially known as the General Secretariat for Community, African and Malagasy Affairs) and the several private or semi-public corporations operating in Africa?

To these all-important questions Corbett's book only offers the vaguest answers. In part this is because the questions to which the author addresses himself are never formulated in these terms, but one also suspects that the sheer inadequacy of his data (most of it drawn from official sources) is a sufficient reason for not raising such questions. In this respect one can only lament the very superficial treatment accorded to the various aid and development agencies, public and private, currently operating on behalf of the Secretariat of State for Cooperation. How these agencies came into being, how their personnel is being recruited, what their policy-orientations and ideological postulates are, and how all this in turn is likely to affect processes of development—these are matters that still remain to be investigated. Equally distressing is the absence of any serious effort to investigate the social bases and ramifications of the top-heavy bureaucratic machinery built around the Secretariat of State for Cooperation. The suggestive findings made by William Cohen in his *Rulers of Empire* (Stanford University Press, 1971), especially as regards the interconnections between the Cooperation establishment and former colonial civil servants, invites a more systematic, in-depth investigation of the kind which one might have expected from a book such as Corbett's.

Perhaps the most disappointing aspect of Corbett's treatment is his neglect of what Suzanne Bodenheimer in a most stimulating discussion calls the "infrastructure of dependency" (*Politics and Society,* I, 3 [May 1971], 327–357). Although due attention is paid to the attitudinal variables which have influenced and continue to influence the receptivity of African elites to France's tutelage, little effort is made to relate these variables to socioeconomic forces. To what extent is the concurrence of interests between African elites and their metropolitan counterparts rooted in class affinities? How dependent politically and otherwise are African entrepreneurs and bureaucrats on French financial, economic, and social resources? To what extent is the dominant position assumed by French economic interests in Africa the key to an understanding of France's political ascendancy in the area? Although these questions go well beyond the geographical scope of Corbett's study, their theoretical relevance to the problems discussed by the author is obvious.

Compounding these substantive shortcomings are a number of questionable statements and inaccuracies. For example: "The implications of growth are foreign to the concept [sic] African societies have of the world" (p. 24); "prostitution was unknown in precolonial Africa" (p. 26); "the conversion to the single-party system that is now general in former Black African territories is undoubtedly more directly attributable to the primitive political environment in which these governments operate than to a French model" (p. 50). In the light of these assertions one must be grateful to the author for not dwelling at further length on the African dimension of the French presence.

Given the aim which the author set for himself ("to undertake an analysis of sufficient detail to provide the basis for an appreciation of the . . . processes through which France exerts predominant influence in the new republics south of the Sahara," p. ix), it is perhaps unfair to take him to task for not doing more than he intended. It would be equally unfair not to recognize the merits of his work. For anyone interested in the impact of French policies in Black Africa, Corbett's book provides essential background information. If for no other reason it must have its place in any library dealing with French-speaking Africa.

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The Concept of Custom in International Law. By Anthony A. D'Amato. (Ithaca and London: Cornell University Press, 1971. Pp. xvi, 286. $9.50.)

According to the traditional view, international customary law is established by the repeated practice of many states if such practice is motivated or accompanied by a conviction that it is required by international law. In other words, the two elements are usage and *opinio juris sive necessitatis.* This technique presents numerous difficulties, especially in a rapidly changing world. How can the law develop if the procedure is rigidly applied? How can changes be employed, determined, and explained without violating existing law? How many, and which, states are required to engage in any given practice, and how can the existence of *opinio juris* be ascertained? Can one or a handful of states exercise a veto, and, if so, can this be done by silence, statements or acts? Can a rule retain its validity if it is challenged by some and defended by others? Is there a distinction between a state's words and deeds? And what is the situation if there is a discrepancy between the two? These are merely some of the questions which can be raised. To overcome these perplexing problems, Professor D'Amato attacks the classical construction and proceeds to reformulate the theory of custom. His formula consists of six requirements. The theory must be internally consistent, general but not vague, applicable to all the inductive evidence, parsimonious, objectively determinable, and claim-oriented. In a larger context the quantitative element of usage is replaced by acts or deeds; and articulation of a rule of international law, which contains several components and is the qualitative aspect of custom, is basically substituted for *opinio juris.* In this regard bilateral and multilateral treaties which contain generalizable provisions may generate rules of customary law and are no different from other acts or articulations which make up custom. Such a formulation "allows for the smooth working of change" (p. 98) in customary law.

Underlying the thesis are the suggestions that international law consists of only a few primary rules and that custom is a secondary rule for finding the content of the primary or substantive norms; custom is the "most important" (p. 44) rule of law determination; it is "at the heart" (p. xii) of the international legal system. The authoritativeness of custom in turn is shown by initiative or imitation, the single act or the legal precedent, and reinforced by three elements—consent, estoppel, and reasonableness—elements which, while severely attacked by the author, are nonetheless accepted because they increase "the sense of legality" (p. 229) of custom.

In presenting what he describes as "a new approach" (p. xiv), Professor D'Amato strongly criticizes much legal thinking, regrettably on occasion in a sketchy or superficial fashion, an observation especially applicable to the McDougal-Lasswell conception of jurisprudence; and he advances certain views, particularly those on the law of treaties, which need further exposition and clarification. These omissions may well stem from lack of space, for the study reveals a good deal of erudition, though the excursion into custom in classical Roman law and English common law, while interesting, adds very little and could have been eliminated. This is all the more true in light of the author's repeated stricture that domestic law analogies are dangerous.

At the same time some statements and illustrations are too ambitious, while others do not stand the test of scrutiny. Among these is the observation that analysis of four cases of the Permanent Court of International Justice and three of the International Court of Justice include "all" (p. 121) the instances from The Hague Courts which indicate that conventional law may generate customary law.

Only much later, in the chapter entitled "The Concept of Special Custom," in which local custom is discussed, is a special rubric for cases on general custom found. Six instances, including two proceedings not mentioned in the earlier section, are recalled, and the claim is made that all of these have been examined in "preceding chapters" (p. 252). Nowhere is reference made to still another ruling, the advisory opinion on *Reparation for Injuries Suffered in the Service of the United Nations,* I.C.J. Rep. 1949 at 174, which reflects on this issue. Similarly, the author all too readily dismisses the holding of the Court in the *North Sea Continental Shelf* cases, I.C.J. Rep. 1969 at 3, that a purely conventional rule concerning the delimitation of the continental shelf had not established a customary regime. And he does so in spite of the fact that the Court knew, at the time of the decision, that the Continental Shelf Convention, 449 U.N.T.S. 311, had received thirty-nine ratifications and accessions and that the equidistance method of delimitation had been employed on some fifteen occasions mostly in the form of bilateral treaties. Nor can it be maintained that the two Courts have adhered "tenaciously" (p. 248) to their precedents. Comparison between the *Eastern Carelia,* P.C.I.J. Ser. B, No. 5 (1923), and the *Peace Treaties,* I.C.J. Rep. 1950 at 65, opinions makes this clear. The language in the *Lotus* case, P.C.I.J. Ser. A, No. 10 (1927) holding that restrictions on the independence of states cannot be presumed on the one hand and the principle of collective fiscal responsibility advanced in the advisory opinion on *Certain Expenses of the United Nations,* I.C.J. Rep. 1962 at 151 on the other, provide a further illustration. Finally, the rulings on preliminary objections in the *Aerial Incident of 27 July 1955,* I.C.J. Rep. 1959 at 127, and the *Temple of Preah Vihear* cases, I.C.J. Rep. 1962 at 6, present additional evidence, as do the *South West Africa* cases, I. C. J. Reps. 1962 at 319 and 1966 at 6. Even more surprising is the assertion that during the height of the cold war, the Soviet Union and the United States "scrupulously" (p. 30) observed international norms on matters such as international airspace and embassies. The several applications on aerial incidents submitted by the United States to the International Court of Justice in the 'fifties attest to the opposite. A more striking example is the very event mentioned by Dr. D'Amato, namely, the U-2 flight. The electronic devices planted by the Russians in the U.S. Embassy plaque in Moscow and subsequently displayed at the United Nations by Ambassador Henry Cabot Lodge should also be noted.

Still, this is a thought-provoking book which should have wide appeal, and undoubtedly others will "join in the dialogue" (p. xiv) which the author has presented.

GUENTER WEISSBERG

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International Dissent: Six Steps Toward World Peace. By William O. Douglas. (New York: Random House, 1971. Pp. 155. $4.95.)

The subject of this book is international cooperation, not international dissent as the title suggests. But this is its major strength. The vast areas of cooperation that lie submerged beneath the waves of international conflict are too often ignored, not only by journalists but also by professional students of international politics. Mr. Justice Douglas in his book properly reminds us that we must concentrate more of our efforts on analyzing the dynamics of international cooperation. Unfortunately, however, he does not succeed in doing this himself.

The author suggests six "propositions" or "steps" toward world peace.

The first five are:

(1) end all military alliances;
(2) abolish all protectorates and colonies;
(3) establish an international regime to regulate exploitation of the resources of the seas and ocean floor;
(4) help developing nations enter the technological age;
(5) establish rules of law governing international relations.

Douglas's sixth proposition, that the United States should recognize the People's Republic of China and that the Communist Government should be admitted to the United Nations as the sole representative of China, has already been achieved to a considerable extent. This latter move was solely within the discretion of the United States Government. The other steps, however, are not, and this points to the main weakness of the book, because the author discusses them as if they were.

It may be, as Mr. Douglas argues, that military alliances not only reflect but conduce to hostility and distrust among nations. There can be no doubt that the world's produced wealth could be better devoted to purposes other than the creation of massive military establishments. But he offers little insight into how to arrive at a world without military alliances. He does suggest that NATO, the Comecon, and the Warsaw Pact nations be united and that all of Europe be transformed into a nuclear free area. But for the most part, exhortation substitutes for analysis.

There is no question, moreover, that the community of nations should exploit the resources of the sea in a fashion that is just, reasonable, and ecologically sane. Nor are there many who would in principle disagree with the goal of enabling poor nations to close the technological gap that separates them from the more highly developed states. But since Mr. Douglas speaks of steps and of policy recommendations and not simply of desirable goals, he must be faulted for not describing the path as well as the destination.

A confusion between means and ends in fact prevails throughout much of the book, as does the absence of that type of middle-range reasoning that focuses on trade-offs, costs and benefits, and utility preference schemes.

These weaknesses become most glaring in Mr. Douglas's own bailiwick of the law. He quite correctly conceives of law neither as a set of immutable principles that exist over and beyond what states do, nor as sanctions imposed upon state behavior by means of punitive force. Laws, for him, are "cooperative regimes," those series of agreements on "procedures to settle differences," that sometimes occur, miraculously, in world politics. In defining law in such a manner, he points to a dynamic concept of law, as have a number of authors like Myres S. McDougal, Wolfgang Friedmann, Louis Henkin and Thomas M. Franck. Mr. Douglas's thoughts, however, at least as reflected in this book, remain rudimentary.

In his outline, for example, of the rules of law that should be adopted by international humanity, he specifies that all questions relating to boundary or territorial disputes as well as those pertaining to territorial and international waters, be submitted to international mediation and arbitration agencies. Another rule of law he advocates is the proscription of the sale of armaments to developing nations. Finally, he would have all governments outlaw invasion. There is nothing in this book, however, that shows how such "legal" rules can be transformed into practical possibilities. It is one thing to criticize the operations of national self-interest; it is quite another to demonstrate analytically how states might be induced to redefine national interests in ways that develop cooperation on a global scale.

The requirements of realism, however, should not obscure the value of this study. Mr. Douglas's book does underscore the fact that international law is largely comprised of restraints imposed by states upon themselves, sometimes even against their immediate self-interest, in order to protect systemic interaction, which is, after all, in the interests of all states. By calling our attention to the importance of systemic forms of international cooperation through and by which more durable laws can be developed, Mr. Douglas has rendered an important service.

EDWARD WEISBAND

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The United States and the Origins of the Cold War, 1941–1947. By John Lewis Gaddis. (New York: Columbia University Press, 1972. Pp. 382. $12.50, cloth; $3.95, paper.)

The Cold War has an ever increasing fascination for scholars. The questions posed by America's failure in Vietnam, recent works by Adam Ulam, Lloyd Gardner, Walter LaFeber, Gaddis Smith, Thomas Paterson, Herbert Feis, Athan Theoharis, Gabriel Kolko, and others, combined with the growing availability of archival and manuscript sources, have established the early Cold War years as the most exciting and controversial period for historians of American diplomacy. While not definitive, John Gaddis's scrupulously researched new book will help bridge the gap between revisionists and traditionalists.

Professor Gaddis accepts some revisionist arguments and rejects others. He follows the view that American desires for a freely trading world ("open door") significantly influenced diplomacy, and his discussion of the Russian loan, lend-lease, and Bretton Woods reflects revisionist historiography. But Gaddis plays down economic motives, stressing instead a more general determination on the part of policy makers to profit from the lessons of the past. Traumatized by the failures of the 1930s, and rejecting isolationism, the Roosevelt and Truman administrations were determined not to repeat earlier mistakes. This time Germany would have to surrender unconditionally. There would be no reparations tangle. This time the United States would join an international organization to maintain peace, even if it required paying court to the sensitive Senator Vandenberg. Secret treaties were taboo; "there would be no 'Colonel House' business," said FDR (p. 13). Collective security and national self-determination were reaffirmed. And balancing this revival of Wilsonian ideals was Roosevelt's firm resolve to continue Soviet-American cooperation into the postwar world, even to the point of compromising certain issues such as Poland. While this American world-view encompassed definite economic values, Gaddis ascribes less importance to them than do most revisionist scholars.

"The most important cause" of the Cold War, according to Gaddis, was the "tension between the American principle of self-determination and Russian security needs" (p. 17). Roosevelt might have been able to square the circle in his own mind, but the public could not. Nor could Stalin fully appreciate the importance that Americans attached to free elections in Poland and elsewhere. Gaddis does not see any sharp break between Roosevelt and Truman on this issue, but he does show Truman as more influenced by anti-Russian advisers and more prone himself to make black-and-white judgments.

The development of American policy toward Germany, the second major focus of Soviet-American conflict, Gaddis argues, resulted not from any conscious design in Washington, but rather from a complex combination of drift, bureaucratic competition, and military momentum. Not until 1946 did American statesmen begin to shape a German policy with Russia clearly in mind. In the case of atomic policy, Gaddis rejects Gar Alperovitz's thesis that Truman and Byrnes negotiated with the bomb on their hips, showing instead a growing realization that exclusive possession of the bomb could work no diplomatic miracles, a genuine desire to consider international control, and a gradual stiffening of the American position because of pressure from Congress. Indeed, in perhaps his best chapter, Gaddis contends that Truman's "get tough" policy actually began in February-March 1946—the result of pressure from influential Republicans like Dulles and Vandenberg, Stalin's famous "incompatibility" speech of February 9, Kennan's "long telegram" of February 22, Churchill's Fulton address, and the Iranian crisis. Thereafter "negotiations would continue, but future concessions would have to come from Moscow" (p. 356).

Were there alternatives to the Cold War? By stressing the role of Congress and public opinion, Gaddis disagrees with most revisionists. Hostility might have been avoided, he argues, had the United States started a second front in 1942–43, granted Russia a sphere of influence in Eastern Europe, given the Soviets a generous postwar loan for reconstruction, and relinquished the atomic monopoly. But neither Congress nor the public would have permitted such policies; hence, in William James's phrase, they were not "live options." Gaddis marshals impressive evidence to show that Roosevelt and Truman worried about congressional and public opinion and that the policies followed evoked a sympathetic response. He does not prove, however, that different policies and more farsighted leadership would not have worked. He plays down the extent to which Presidents can ignore Congress and manipulate public opinion, as evidenced by Vietnam. What Gaddis demonstrates is that a real Cold War consensus developed early, shared by statesman and man-in-the-street alike. There might have been more viable alternatives for American diplomacy in the Far East, about which Congressional and public opinion was less developed, but Gaddis deals with Asia only briefly.

The book is well-written and carefully documented, with notes at the bottom of the page. Although there is no bibliographical essay, Gaddis often makes incisive comments of a historiographical nature in the footnotes. It is a book for historians, but no political scientist interested in the Cold War can afford to ignore it.

J. Garry Clifford

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The Foreign Powers in Latin America. By Herbert Goldhamer. (Princeton: Princeton University Press, 1972. Pp. xiv, 322. $10.00.)

The United States and the Latin-American Revolution. By Martin C. Needler. (Boston: Allyn & Bacon, Inc., 1972. Pp. viii, 168. Price not indicated.)

One lesson to be learned from reviewing these books jointly is that it is not necessarily true that—as this reviewer shamefacedly admits he once believed—the stranger the bedfellows the more interesting, let alone exciting, the bed. The matter is not always that simple.

In *The Foreign Powers in Latin America,* Herbert Goldhamer has performed an interesting, useful, and possibly needed service. We live today in a world predominantly preoccupied only with the role of the United States in areas like Latin America. Goldhamer reminds us that Washington's is not the only finger in the inter-American *puchero.* There are other meddlers, other investors, other imperialists; and many of them are European. Even where diehards insist that the United States is the largest and most influential of these actors, Goldhamer at the very least provides us with a perspective for evaluating the range of the entire company of foreigners who complicate and otherwise influence the lives of Latin Americans.

We are therefore reminded that in the Caribbean there is not only a Puerto Rico but also a British West Indies, a French West Indies, and a Dutch West Indies. We knew that all along, to be sure; but a message of this undertaking is that it is sobering to be reminded of non-American influences in the Western Hemisphere. In assessing European and Japanese activities in Latin America, Goldhamer points out that a difference between those "imperialists" and the United States is that in general the former are not as concerned with national security considerations in Latin America as are the "North Americans." Soviet interests in the Caribbean immediately jump to mind. Here Goldhamer offers the interesting and probably valid judgment that the long-run consequences of Soviet involvement in Cuba have included a general cooling of Latin-American pro-Castro sentiment outside of Cuba without any significant lessening of anti-United States attitudes in the Americas.

On the economic front, Goldhamer's study shows that, after all these years (his data begin with 1938), the chief non-American trader with Latin America is still Germany, with postwar Japan now in second place. The author explores the possible role of migration as an instrument of "foreign" influence in Latin America, but founders on the curious, long-known, but still little-understood proposition that most of the American republics have never attracted significant quantities of European immigrants. In the Americas Argentina has been second only to the United States in the exercise of this magnetism; otherwise, remarkably small percentages of Europeans have been persuaded to emigrate to the Americas over the years.

Goldhamer's data lead him to the conclusion, heavily economic in emphasis, that, in the years since World War II, the "foreigners" who have been most successful in achieving their objectives in Latin America have been the Germans and the Japanese; that the Soviet Union has been moderately so; and that the United States has been the principal loser.

Martin C. Needler's little book, *The United States and the Latin-American Revolution,* in one sense documents Goldhamer's proposition regarding "North American" losses in the Western Hemisphere. In another, and more fundamental, sense, Needler marches to a different drum. His work seems to be made up of more units of nostalgic ideology than of social science or—forgive the expression—realism. The book reads as though it were written in the late 1950s. Kennedy-type liberalism and faith in the Alliance for Progress are much in evidence. A similarity between Needler and me is that both of us were Kennedy Democrats. A difference is that he appears to believe that, having missed one boat, we should take it anyway. Would it really be cynical to attempt to book passage on a later vessel?

In any case, the contrasts between the two volumes are impressive. Goldhamer's is a serious study, a major undertaking; Needler's is a little book. The former is a systematic inquiry, employing reasonably scientific techniques and controls; the latter partakes more of the nature of an ideological statement. The former is intensively researched, despite Goldhamer's protestation that his is an "ignorancia casi enciclopédica" (p. vii).

President Nixon's name is invoked more by Needler than by Goldhamer. This is to be expected for a number of reasons, the most obvious being that one of these books is about the policy of the United States, and the other about the activities of non-American "foreigners," in Latin America. In addition to Needler, other students of the Nixon Administration have noted that Latin America is on the "back burner"—i.e., given only secondary attention—so far as present-day "North American" policy is concerned.

Despite the differences and divergences between the two books under review, they join together in underscoring the secondary character of their subject matter. Goldhamer takes advantage of this hiatus to give us a well-researched perspective, demonstrating that in today's world the various American republics are on everybody's—including the non-American "foreigners' "—"back burner." This is enough to make Latin Americanists scream in anguish. But it is probably true. Whosoever, even Goldhamer, tells us the truth performs a constructive service. And Needler's message appears to be that we are still waiting for the boat we missed. To paraphrase an old anecdote, in one case the situation is serious but not desperate; in the other, it is desperate but not serious. Such are the ways of the "back burner."

Here are two books about the place of Latin America in today's world of international politics. Together they tell a story of secondary emphases and missed opportunities. Fundamentally, it is a true story. Read it and weep.

GEORGE I. BLANKSTEN
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Soviet Public International Law: Doctrines and Diplomatic Practice. By Kazimierz Grzybowski. (Leyden: A. W. Sijthoff; Durham, N.C.: Rule of Law Press, 1970. Pp. 544. $18.50.)

This book is the first English-language work since 1935 to analyze in comprehensive fashion Soviet doctrines and practice in international law. Kazimierz Grzybowski, Professor at the Duke University School of Law, provides systematic treatment of Soviet views on international-legal theory, the state and recognition, jurisdiction, population, diplomatic and consular agencies, international organizations, treaties, and dispute settlement procedures. Drawing on his rich knowledge of international relations, particular as among East European states, Grzybowski illustrates Soviet doctrinal positions with numerous examples from Soviet diplomatic practice. He thereby keeps the book from becoming a dreary repetition of opinions of Soviet international-law publicists. Grzybowski's extensive utilization of diplomatic practice is the book's strongest feature.

Professor Grzybowski's work, however, contains shortcomings of both a technical and judgmental nature. Grammatical and stylistic errors make the book difficult to follow at many points. The book's bibliography omits many English-language works that would be useful to a reader desiring to pursue more closely the topics covered briefly in Grzybowski's survey analysis. The book's title is open to question, since there is, in fact, no such thing as "Soviet public international law." Each country does not have its own public international law, but rather a set of doctrines and a history of practices with respect to a universal set of legal precepts. A title along the lines of Soviet "views" or "approaches" to public international law would be more accurate. In two instances, Grzybowski cites repealed enactments as representing current law. He cites the 1929 Soviet Merchant Shipping Code rather than the 1968 Code that replaced it (pp. 172–173, 174, 255) and the 1958 Comecon General Conditions of Delivery rather than the newer 1968 General Conditions (pp. 413–414).

Certain of Professor Grzybowski's judgments about the legality in international law of Soviet actions are questionable. He states that the Soviet refusal to recognize a right of innocent passage for foreign warships in Soviet territorial waters constitutes a violation of the 1958 Geneva Convention on the Territorial Sea and the Contiguous Zone, though most authorities agree that this Convention left that question unresolved (pp. 152, 158). In discussing the 1962 Cuban missile crisis, Grzybowski ignores the crucial legal issue, namely, the doctrine of "anticipatory self-defense" employed by the U.S. State Department to justify the U.S. blockade against Soviet missiles, and concludes, without foundation, that the Soviet removal of missiles from Cuba indicated a recognition by the U.S.S.R. of the legality of the U.S. blockade (pp. 177–178). In analyzing Soviet treatment of aliens, Grzybowski opines that "powers of the administrative authorities and courts in the Soviet Union are so vast that any action may be justified by adherence to the codes and regulations in force" (p. 85). Such an observation is more than a trifle exaggerated.

Professor Grzybowski underestimates the Soviet contribution to contemporary international law when he concludes that "the overall effect of Soviet doctrines on the organization of the modern community of nations is not as general as Soviet jurists and diplomats would like the general public to believe" (p. 519). Commenting on early Bolshevik departures from traditional rules of international law, he suggests: "From the perspective of the present day, however, acts of the revolutionary government seem less radical as regards the transformation of the foundations of international order than they were proclaimed to be at the moment of the Revolution" (p. 100).

If those early departures seem less radical today, it is only because many of them have been accepted in the world community. State ownership of the means of production is now practiced in many nations to a greater or lesser extent, and this concept has caused revision of many traditional rules of international law. Similarly, the Soviet "pick and choose" approach to treaties concluded by the old regime following a social revolution has been pursued by many Asian and African states upon attainment of independence. Likewise, the wary attitude of these former colonies to international adjudication and to international organizations stems from the same motive that engendered Soviet uneasiness in these regards—a distrust of institutions developed by the West European states, staffed predominantly by West Europeans, and operated on West European principles. Thus, many current Third World attitudes toward international law find their first expression in Soviet statements and practices.

The People's Republic of China and the other Asian and East European socialist countries have also emulated Soviet doctrines and practices across a broad range of issues in international law. More generally, Soviet insistence on the use of broadly-based multipartite conventions has gone far toward undermining the traditional predominance of custom as the major source of international law.

Though Professor Grzybowski's book suffers from these shortcomings, it provides much useful information and much incisive analysis of Soviet views on international law, and it fills a long-standing gap in the legal literature.

JOHN QUIGLEY

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The Beginning of American Aid to Southeast Asia: The Griffin Mission of 1950. Edited with introductory chapters by Samuel P. Hayes. (Lexington, Mass: Heath Lexington Books, 1971. Pp. 334. $15.00.)

As a major new effort in foreign assistance to Indochina impends, it is timely and interesting to read Samuel Hayes's compendium and evaluation of the start of the first post–World War II aid efforts in Southeast Asia in 1950. The 1950 effort was launched by the visit to Southeast Asia of a U.S. government mission headed by Allen Griffin. Mr. Hayes was the deputy head of the mission. Coming in the wake of the Communist victory in Mainland China, the mission had the task of finding suitable projects for U.S. assistance that would "have immediate political significance," as well as laying the groundwork for an anticipated economic development program "of unknown dimensions" in the future.

To this end, the Griffin Mission visited Vietnam, Malaya, Burma, Thailand and Indonesia during March and April, 1950. Its reports and recommendations, which are published for the first time in this volume, proposed economic aid for the five countries totaling $66 million initially, concentrated principally in agriculture (about 35 per cent), with smaller amounts in other fields including power and transportation (26 per cent), and public health (24 per cent). The Griffin Mission's recommendations were implemented with remarkable dispatch. Within a matter of months, country missions were on site in the several countries to start the programs, and to establish the procedures and projects which provided the basis for the continuing assortment of U.S. aid programs in Southeast Asia.

Mr. Hayes notes this record of speedy implementation with pride: "The fact remains that prompt action was desired and prompt action took place. It is a history not likely to be often repeated" (p. 58). It is not unreasonable to consider in retrospect whether one ought really to wish for repetition: perhaps more thought, caution, and deliberation need have turned out no worse.

The materials that are published in the Hayes volume are valuable reference sources. They reveal plainly the implicit premises that underlay much U.S. foreign assistance in the post–World War II era. The premises and implicit theories include: the "strategic importance" of Southeast Asia; the strength of internal nationalism as a source of political support in the Southeast Asian countries; the international and domestic political effectiveness of foreign economic aid; and the important catalytic role of foreign aid in internal economic development. These premises were not necessarily wrong in the 1950s, although they are, to put it mildly, unpopular in the 1970s. Nor, at the same time, can it be said that they have been fairly or critically analyzed, let alone tested—then, or since.

In light of subsequent experience, some of these premises look sounder two decades later than others. Yet even the ones that look sounder are oversimplifications; on the other hand, even the ones that seem *less* sound by hindsight, have some validity, too. What the premises share is a tendency to assert single and simple explanations for phenomena that have multiple and complex causes.

On the eve of the new programs of reconstruction and development in Indochina, a review of the Griffin Mission reports has particular timeliness. One cannot be sure of what lessons should be drawn from this background, but a few may be worth considering as the new aid effort gets under way: Don't expect too much from the programs, either economically or politically; try to be clear about criteria—what is aimed for, and what is reasonable to expect; try not to make it a U.S.-dominated "show"; make the number of U.S. personnel as limited as possible.

CHARLES WOLF, JR.

The Rand Corporation

Law and Policy in China's Foreign Relations: A Study of Attitudes and Practice. By James C. Hsiung. (New York: Columbia University Press, 1972. Pp. 435. $12.50.)

Although much has been written about the PRC's international relations, insufficient attention has been given to her attitude and practice with respect to international law. The present volume is thus especially welcome, for it addresses itself to the question, "To what extent does Communist China, armed with a militant ideology, accept the existing rules of the game in international relations?"

Composed of fifteen chapters, the book analyzes a wide range of vital issues concerning law and policy in China's foreign relations. It shows that international law has been used either to facilitate Peking's foreign policy, to support its claims to certain international rights, to protect Chinese interests and nationals abroad, to defend certain official conduct, or to help the carrying out of trade or other similar relations.

A professor of politics at New York University, the author writes with commendable objectivity and perceptiveness in his effort to evaluate the interaction of law and policy in the PRC's decision-making process. He correctly suggests that both in theory and practice, China has been quite selective and more pragmatic than is generally assumed, accepting certain norms of the established code of international conduct but rejecting others. According to him, the widening gaps (1) between the PRC's doctrinal bellicosity and pragmatic cautiousness, and (2) between her ideological identification with the socialist bloc and her expanding ties with countries outside the bloc, signify an evolutionary (as opposed to dogmatically immutable) character of her foreign policy. Nevertheless, he cautions, it may take years before Peking can be expected to attain a level of maturity and flexibility that we would like to see.

One of the highlights of the book is its treatment of peaceful coexistence (PCX) and proletarian internationalism (PI), two fundamental principles of "modern" international law endorsed by the PRC and the Soviet Union. Through a careful analysis of their development over the years, the author points out that PCX and PI have always served the policy needs of Peking as well as Moscow, that the evolving meanings of the two doctrines have always followed the changing policy needs of the decision makers, and that Sino-Soviet divergencies over the application of the two doctrines have always reflected deeper differences in policy.

Equally interesting is the book's examination of the PRC's position in regard to the sources of international law. While there is little doubt about the PRC's recognition of treaties and custom as the major sources of international law, considerable confusion has existed about her attitude toward the resolutions of international organizations. The author successfully clarifies the picture by showing that Peking accepts certain decisions of international organizations as a source of international law so far as they embody a construed *opinio juris* (representing the consensus of a majority of the "nonimperialist" countries and subscribed to by the PRC herself).

On the whole, *Law and Policy in China's Foreign Relations* is a well-researched and well-written book. There are, however, some shortcomings. The organization of the volume, for instance, leaves something to be desired. To make the flow of discussion more smooth, Chapters IX and X on practical use of international law probably could have been either integrated into the other chapters or put at the end of the book. One also wishes to see a more detailed analysis of the PRC's stand on airspace and on the principle of self-determination. Another inadequacy appears to be the absence of any discussion of Peking's approach to the general question of human rights.

These "fringe" criticisms do not distract from the important contribution made by the book to our understanding of the PRC's foreign policy and world view. This volume should prove valuable to Chinese specialists and international lawyers alike.

SHAO-CHUAN LENG

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Development Today: A New Look at U.S. Relations with the Poor Countries. Edited by Robert E. Hunter and John E. Rielly. (New York: Praeger Publishers, 1972. Pp. x, 286. $9.00.)

These papers—some of which have been published elsewhere—were originally presented at seminars of the Overseas Development Council (ODC). They vary considerably in terms of depth of analysis and sophistication.

The most provocative piece in the volume is "Foreign Aid: For What and for Whom," by Samuel P. Huntington. According to Huntington, "true believers" in foreign aid have done their cause a disfavor by repeated and fumbling attempts to find a grand rationale for foreign assistance. Huntington argues strongly that long range economic development of the Third World is not so important to U.S. interests that it ought to be the primary goal of our foreign aid programs. There is little evidence that a United States commitment to development of the Third World will bring political gains to the U.S., either in the short run or the long run, by strengthening political alliances or winning new allies. On the contrary, strains which arise in bilateral assistance relationships often have negative implication for U.S. foreign relations.

Huntington argues for a multi-objective approach to foreign assistance and a decentralization of aid programs. Assistance for long range economic development should be the function of multilateral development institutions such as the World Bank and the regional development banks. According to Huntington, legitimate objectives of bilateral aid, which should be administered by the State Department, include the following:

(1) Granting of economic assistance to a friendly government to help it consolidate its position after coming to power, survive temporary economic crises, or win a crucial election; or

(2) Granting of assistance to a government in return for something we want such as base rights, a U.N. vote, military cooperation, freer access to the country's markets for U.S. exports or U.S. investments, or maintenance of a cease-fire agreement.

Military assistance, says Huntington, ought to be entirely a Defense Department matter. Separate authorizations and appropriations, rather than an omnibus Foreign Assistance Act, should cover the various elements of the program. Unfortunately, Huntington gives little attention to humanitarian and technical assistance, the focus of much of AID's current activities. His proposed dismantling of AID would leave these programs in limbo.

Huntington's essay is followed by an article by Willard Thorp, entitled "Foreign Aid: A Report on the Reports" which describes the evolution of thinking on foreign aid by various official commissions, the kind of thinking that Huntington feels is so misguided.

Robert Hunter discusses United States interests in Asia and the Middle East, the problem of violence in the Third World, and participation by LDC's in international cooperation on monetary and trade matters. John Rielly focuses on Latin America, the tension between United States and European roles in that part of the world, and U.S. security and trading interests in Latin America. Neither the Hunter or Rielly essays contains strident calls for foreign assistance; rather they represent low-key efforts to emphasize the extent of U.S. interests in the Third World, with suggestions as to the role that development assistance might play in furthering these interests.

The book ends with a section on the U.S. Congress and foreign aid. Representative Donald Fraser from Minnesota explains the lack of congressional support for aid as due not only to the Vietnam backlash and increasing concern with domestic problems, but also to the significant reduction in the last decade among aid-supporting liberals in Congress. Senator Frank Church outlines his opposition to U.S. bilateral, foreign-aid programs on the basis of what he regards as false premises advanced by aid advocates: (1) the existence of a Communist monolith that has to be checked in its imperialistic drives, and (2) the belief that foreign assistance can prevent a country from "going Communist." He also argues that our aid programs support undesirable dictatorships, maintain the status quo when radical change is required, and line the pockets of American exporters and contractors. Robert Hunter concludes the volume by rebutting Church's arguments, writing that no respectable proponent of foreign aid in the last decade has used the Communist monolith argument or maintained that aid provides political guarantees. He does not see radical political change as a necessary precondition for economic development, and adds that exporter and contractor profits derived from foreign aid programs need not demean their usefulness to the recipient countries.

A number of other essays in the volume deal with trade, the multinational firm, violence and development, population growth and environmental stress, and employment and income distribution. One of the most interesting of these is an article by James Grant, "Jobs and Justice: Economic Growth Alone is Not Enough." Mr. Grant points out the vastly different levels of performance among countries in reducing unemployment, improving income distribution, and providing educational and health services for the lower economic strata within developing countries. He notes that the progress of some rapidly growing countries in achieving social goals would not have been possible without economic growth, and that success is often associated with broad access to foreign assistance, international trade, and foreign investment.

The articles in this collection cover a wide range of topics and vary in quality and interest. It is difficult to find a rationale for publishing them in a single volume. Nevertheless, the book is a useful addition to the library of anyone interested in foreign aid and economic development because of its five or six best essays.

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The Cyprus Question. By Leontios Ierodiakonou. (Stockholm: Almqvist & Wiksell, 1971. Pp. 311. Kr. 30.)

The study of international disputes can be pursued either on a substantive level aiming at the illumination of particular conflicts or on a comparative and theoretical level with individual cases studied in order to clarify questions of general significance. To be sure, no firmly founded comparative and theoretical projects can be undertaken in the absence of reliable case studies which therefore are always pertinent.

Mr. Ierodiakonou's book on the Cyprus Question falls within the first category of international studies. It is a work of good scholarship offering a balanced, well-reasoned and comprehensive account of the international activity provoked by the nationalist revolt in Cyprus in the 1950s. The narrative suggests that what began as a straightforward demand for self-determination on the part of the Greek Cypriots ended up, after increasing internationalization, in political deadlock evolving out of: (a) bad timing and poor tactics on the part of the Greek side; (b) manipulative British policies dictated by preoccupations with Middle-eastern oil resources and the exigencies of the Cold War; and (c) the Turkish government's efforts to cope with popular discontent by diverting attention from difficult conditions at home to nationalist grievances abroad.

Repeated recourse to the United Nations did not provide a way out of the stalemate which was further aggravated by continuing violence in Cyprus. Active diplomatic pressure in the interests of the Western Alliance led to the compromise of the Zürich-London Agreements (1959) among the Greek, Turkish and British governments, setting up the Republic of Cyprus. The outcome essentially amounted to abandonment of self-determination by the Greek Cypriots, while at the same time it constituted a net gain for Turkey and attainment of the British policy objectives (pp. 222–227).

Although the main focus of the study (chaps. 2 to 8) is on the international dispute of the 1950s, a substantial Epilogue (pp. 228–300) covers the application and collapse (in 1963–64) of the Zürich-London settlement as well as subsequent political developments in Cyprus. An attempt at a profile of local political dynamics (e.g., political alignments, interest groups, foreign influences, etc. (pp. 275–300), suggests the importance of these factors for a sound understanding of the Cyprus Question.

In his otherwise useful survey of the literature on the politics of Cyprus (pp. 14–17), Mr. Ierodiakonou should have cautioned us that many of these books are biased statements of divergent points of view rather than reliable factual accounts. The documentation of the argument, though detailed and careful, suffers from some discrepancies. The most interesting source explored is the *Hansard*. Had the author also drawn upon the relevant Greek and Turkish parliamentary debates, his argument would have been more complete. As he claims on p. 17 it is indeed harder to obtain Greek and Turkish sources but this is not a serious excuse, especially since other scholars have already made fruitful use of the proceedings of the parliamentary bodies of Greece and Turkey. (Cf. for example Th. Couloumbis, *Greek Political Reaction to American and NATO Influences* [New Haven and London: Yale University Press, 1966]; C. H. Dodd, *Politics and Government in Turkey* [Berkeley and Los Angeles, University of California Press, 1969]; and more recently P. Terlexis, *Diplomacy and Politics of the Cyprus Question* [in Greek], [Athens: Editions Rappas, 1971.])

In the framework of the historical-juridic approach employed in this study, an attempt is made to discuss the Cyprus Question with reference to wider international issues such as the interpretation of the U.N. Charter and the application of the principle of self-determination. The discussion of this concept (pp. 40–44) is a good one, but it could have been much more pertinent if the author had not ignored certain important contributions on the subject (for example, W. Conner, "Self-Determination: the New Phase," *World Politics*, Vol. 20 [October 1967], 30–53, and V. Van Dyke, "Self-Determination and Minority Rights," *International Studies Quarterly*, 13 [September 1969] 223–253).

In general the author does not show any awareness of contemporary analytical approaches to international relations—approaches which could have provided remarkable insights into the problem of Cyprus. An initial effort to present the dispute as the outcome of the social and political forces at work in Cyprus, Greece, Turkey and Britain (chap. 1) is a good attempt in the direction of historical sociology, but the final product falls short of one's expectations. In particular the treatment of the nationalist *Enosis* movement in Cyprus, which has been a major driving force complicating the Cyprus Question, is quite unsatisfactory (pp. 18–23).

As a result, the analysis remains on the level of external facts (i.e., official policy) without coming to grips with the dynamic nature of the problem. An international dispute can be understood through an analysis of the factors that make it difficult for ethnic groups and nations to coexist in peace. Such sources of conflict include differences in culture and historical experience, psychological tensions, clashes in nationalist aspirations, socioeconomic antagonisms and political incongruities, discrepancies in levels of modernization and so on. Cyprus is a microcosm of broader Greek-Turkish conflicts, a microcosm in which several of these dynamic patterns can be studied with fruitful results both on the analytical and the policy level. For example, a systematic typology of sources of conflict could be developed to serve as the basis for the formulation of resolution strategies. In this way the study of the Cyprus Question would certainly acquire much wider interest.

One may conclude by noting that Mr. Ierodiakonou's book, though a reliable case study factually, does not carry us into new territory in analysis and interpretation.

PASCHALIS M. KITROMILIDES
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The Oder-Neisse Boundary and Poland's Modernization: The Socioeconomic and Political Impact. By Z. Anthony Kruszewski. (New York: Praeger Publishers, 1972. Pp. xviii, 245. $16.50.)

World War II and the arrangements which followed immediately afterwards shifted the geographic location of a resurrected Poland dramatically westward. Six million Poles, the majority of whom resided in the rural East which was ceded to the Soviet Union, were moved to the Western and Northern regions acquired from a defeated Germany. Professor Kruszewski's book deals with the economic, demographic and sociopolitical changes which resulted from these shifts, and with their impact both within the "new" territories and for the nation as a whole.

This is a meticulously documented study based upon statistical and archival evidence, some of it previously unknown. In order to piece his story together the author had to overcome the usual difficulties encountered by an "outsider" doing research in Eastern Europe and the Soviet Union, not the least of which is often the lack of reliable statistical data, especially for the early postwar period. That he has managed so well is a credit to his patience and ingenuity.

Although the boundary changes, especially those involving divestiture of the Eastern territories, met with the resistance of the Poles (with the exception of the Communists), these have ultimately turned out, in the view of the author, to be beneficial to the sociopolitical and economic life of the nation. If within the economic framework of prewar Germany the territories acquired by Poland were considered relatively backward industrially, their acquisition marked a giant step for Poland on the road toward industrialization, urbanization, and technological development. According to Kruszewski, this, more than the official ideology and the concomitant sociopolitical revolution, has contributed to the transformation of Poland and to the changed character of its population. In addition to attracting those resettled from the East, the new territories served as a magnet for the rural youth who could not find a future on the traditional Polish dwarf farm, for first generation proletarians, and for re-immigrants from Western Europe and the United States. Others were forcefully resettled into the Oder-Neisse regions. The new settlers took up residence alongside indigenous residents, the so-called autochthons, that is, ethnic Poles—however strongly Germanized—who always lived in these areas. The newcomers have taken the places vacated by ethnic Germans who either fled or were forced to move into a partitioned Germany.

According to Kruszewski, integration of the new territories into the rest of the country appears to have been smoother than its own postwar socioeconomic and political stabilization. The natives to the area, for example, the aforementioned autochthons, although having suffered under the preceding German administration and longing for a return to the Polish homeland, had envisioned a Poland far different from that which emerged from the ashes of World War II. They were old-style patriots, devoutly religious and conservative, and felt out of place in a militantly secular "people's democracy." Moreover, their dialect, heavily laden with German phrases, and the past service of their menfolk in the German armed forces, made them suspect in the eyes of the newcomers. Many of them are currently migrating to West Germany under terms of the Warsaw-Bonn treaty. The economy itself suffered destruction since the Soviet armies moved machinery and entire plants, as war booty, before handing over the region to the Poles.

In this situation of flux, the author maintains, the Church served as the mainstay of order and continuity while the Party, although lacking in broad social support and influence and frequently forced to seek the moral assistance of the Church, offered organizational and managerial muscle. In fact, the economic needs, on the one hand, and the unpopularity of the dominant Party, on the other—coupled with mutual mistrust—offered many otherwise politically "hostile" elements opportunities for developing their energies and talents along non-political lines, e.g., in technical management, education, non-Party activism, etc.

Kruszewski somehow refuses to draw generalized conclusions beyond his narrow subject-matter focus, although these suggest themselves almost naturally. More general conclusions, however, would have made the book more important to the reader interested in international politics, especially as it affects problems of development.

Thus, what becomes obvious is that minor states are very much at the mercy of the big powers, the megalomania of their native elites notwithstanding. Another conclusion concerns the use of boundary changes and population shifts as instruments in international politics. It was Winston Churchill who as early as 1944 advocated mass population expulsion as a pragmatic and just course of action. Kruszewski cites him as saying: ". . . expulsion is the method which, so far as we have been able to see, will be the most satisfactory and lasting. There will be no mixture of populations to cause endless trouble as in Alsace-Lorraine. A clean sweep will be made" (p. 23).

Generally, a country such as Poland, economically underdeveloped, emerging from near total destruction in the aftermath of an exhausting war, has politically and economically two courses of action open to it in the early postwar years, neither of which is very appealing: It can turn to foreign capital for assistance in the task of reconstruction, thereby becoming an economic as well as a political colony, or it may attempt to exploit its own resources, including its own manpower, via a system of mobilization and regimentation. If the first alternative is rejected, the second may serve as an excuse for the type of repressive regimes which emerged in the immediate postwar years in Eastern Europe. In fact, the author reproduces in an appendix a hitherto unpublished OSS document which suggests that a "rigorous dictatorship would be . . . the most likely political framework of an economic policy involving changes as radical as may be necessary to lead Poland out of its prewar stagnation" (p. 212). The same document also maintains that "good relations between the United States and the USSR is the most important single condition" in the postwar period and, therefore, East European countries whose fate will be decided ultimately in the chancelleries of the great powers, have little to hope for by pushing for a wedge between East and West.

The author overstresses perhaps the impact of the boundary changes at the expense of other change-inducing factors. Unquestionably, these changes and the desire to make these changes permanent have aided in gaining legitimacy for the postwar Polish system in the popular consciousness, and to the extent that the current leadership (and the Soviet Union) can portray itself as the major guardian of the present status quo (especially as related to the new national frontiers) it will enjoy mass support, although without love or admiration. While Professor Kruszewski evidences some ideological biases, his treatment of the subject is admirably sober, generally understanding and sympathetic although somewhat critical. It is refreshing because it departs so markedly from the emotionalism characteristic of much of the "emigre literature" when dealing with the problem of Polish frontiers, East and West.

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Forecast for Japan: Security in the 1970's. Ed. by James W. Morley. (Princeton: Princeton University Press, 1972. Pp. 249. $9.50.)

Although writing independently in this volume, the six authors of seven chapters began their study as an analysis for the U.S. Arms Control and Disarmament Agency. That is probably why they worked within the time-frame 1970–75. This allowed projections to be rather firm, yet in certain techno-industrial fields (nuclear reactors, fuel enrichment, missile and space capabilities, air and naval expansion) and related strategic options, projections to 1980 and '85 would have been helpful. There is some of this longer-range thinking in Donald Hellmann's chapter (V) on Japan's Asian environment and defence choices, in Morley's concluding "Forecast," and—by implication—in Kenneth Young's reasoned foresight as to Japan's interests and probable policies in Southeast Asia (Chapter VI).

Although Martin Weinstein's chapter (II) provides a history of conservative Japanese diplomatic and security policies since 1947, the book opens with Morley's brilliant depiction of the calculated economic parameters within which Japan's rearmament has been and will in the future be conducted. The particularly fresh contribution provides data about the significance of gross incremental capital-output ratio during 1959–68 and as estimated for 1969–75, Morley concludes that in terms of what that country's economy can sustain, its leaders (conservative during the foreseeable future, write Nathaniel Thayer and Frank Langdon in the third and fourth chapters) have the following options: (1) they could continue spending less than 1 per cent of GNP (under 8.5 per cent of total governmental expenditures) on military items and so attain middle-power status by the mid-'seventies; or (2) just from economic considerations they could (by investing about 2 per cent of GNP) make their country a major power; or (3) with much more strain, controversy and international suspicion—and over a longer span—they could even match their status as an economic superpower in military fields.

All of the writers except Hellmann, whose analyses and projections are more "hard-nosed" and deterministic, think that Japanese conservative governments will continue to prefer economic and increasing political emphases in diplomacy and continuation of the first-mentioned pattern of security measures. This would enable Japan to develop much more adequate conventional defenses of its home islands. Only Hellmann has expressed here a strong prediction about probable Japanese responses to any serious threats to its vital use of sea lanes (i.e., that in the future Japan will probably see the need to protect them). The other authors think that, if the United States is to play a significant though reduced role in East and Southeast Asian affairs, Japan will continue for some time to depend on the U.S. nuclear shield, and on offshore American air and naval power. Any serious reason to doubt American will to fulfill the responsibilities of its alliances in that area and the Western Pacific would tend to cause Japan to change its policy options. Hellmann is probably correct in positing that the more distant time when the People's Republic of China will probably become capable, by an array of ICBMs, of placing the continental United States under nuclear threat will be a crucial one for Japanese strategic policy makers.

Allan B. Cole

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International Conflict and Social Policy. By Marc Pilisuk. (Englewood Cliffs, N.J.: Prentice-Hall, 1972. Pp. 199. $5.95.)

In this slim volume the author, a professor of social welfare at the University of California, Berkeley, presents a subjective and poorly documented interpretation of the nature and purposes of American Foreign policy. His analysis covers such diverse topics as the origins of strategic thinking and its impact on the policy process, the role of the CIA in foreign policy, the exploitation and subjugation of the Third World, and the impact of the military-industrial complex on foreign policy.

The central thesis of this work is that the U.S. has developed a vast military and paramilitary foreign policy which reflects the interests of powerful corporate and bureaucratic factions. Many of the policies of these factions are designed to support reactionary anti-Communist regimes favorably inclined to the protection of American economic interests.

A reader familiar with the current debate over the nature and purposes of American foreign policy will recognize this thesis as representative of the "revisionist" interpretation of American foreign policy. On balance it adds very little to the elaboration of the revisionist argument and even less to our understanding of American foreign policy.

In his introduction the author informs the reader of his preference for the analysis of "concealed data" as opposed to the "systematic ordering of easily available data" (p. 4). The methodological problems in the use of concealed data are considerable, and unless handled carefully, can pose significant problems for the author. The author does not discuss what he means by concealed data, and his method of dealing with them does little to inspire confidence in the objectivity of his analysis.

In particular, he fails to provide the reader with information on the method of inquiry followed in uncovering the data. As a direct consequence of this failure, the reader has no way of determining if the methods of inquiry are consistent with the canons of scientific method. One gains some insight into the methodology of the author by noting that the references listed consist almost entirely of secondary sources. Pilisuk does not indicate what information is distorted or concealed. Moreover there is no serious effort to document distortion and concealment.

The second major methodological weakness is Professor Pilisuk's tendency to assert factual statements which appear at first glance to raise fundamental questions but which upon further analysis are poorly supported by the data presented. This failure can be seen in the following examples:

In his analysis of the policy of the United States toward the United Nations he contends that "American domination of the United Nations is as pervasive as before" (p. 18). The evidence presented for this rather controversial assertion is the role of the UN in supporting U.S. policies in Korea and Hungary. These cases are supposed to prove the case of domination, and they are cited as typical of the past record. Yet the cases cited are from the 1950s. Is the author unaware of the profound changes which the UN has undergone since 1956? The author might have cited cases of comparable control over the UN in the 1960s. Or better still, he might have provided the reader with measures of the degree and scope of U.S. control over the years and across a range of issues. As it stands his thesis lacks adequate supporting data.

In his discussion of the policies of the United States in the Third World, Pilisuk asserts that "greater foreign capital imports are correlated with lower rates of economic growth" (p. 36, emphasis in the original). This thesis runs counter to the central premise of much of the literature on economic development and foreign economic assistance and, if substantiated, would constitute a significant finding. There are no data which demonstrate the statistical correlation between economic growth and capital imports as asserted in the quotation. In the absence of such data this conclusion cannot be accepted as an objective and verified description of the relationship between imports of capital and economic growth.

Pilisuk offers case studies of American interventions in Greece and the Dominican Republic. His descriptive narrative of these cases adds little to the existing knowledge on these past events. The accounts rely heavily on secondary sources and do not reveal any new data. The interpretations are very one-sided and the explanations offered are oversimplifications. For example, the Dominican intervention is explained by reference to the ideological predispositions and economic interests of the principal policy makers. Important considerations such as the Johnson administration's concern over the domestic and international ramifications of the insurrection are not examined. These variables have no place in an economic interpretation of American foreign policy and are apparently rejected out of hand by the author.

On the plus side, the book contains interesting discussions of the military-industrial complex and the impact of strategic thought on strategic policy. These chapters may be of some interest to the general reader, although the specialist will find them of marginal value.

JOHN W. ELEY

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Southern Africa in Perspective: Essays in Reional Politics. Edited by Christian P. Potholm and Richard Dale. (New York: The Free Press, 1972. Pp. 418. $12.95.)

South Africa and the World: The Foreign Policy of Apartheid. By Amry Vandenbosch. Lexington: The University Press of Kentucky, 1970. Pp. 303. $8.50.)

Southern African affairs are increasingly drawing the attention of social scientists as they search the globe for issues and action. If nothing else, Southern Africa has something for everyone—racial conflict, guerrilla insurgencies, authoritarian governments, strategic materials, valued geopolitical terrain, and lots of capitalist investment. These two books, to one degree or another, take up most of these issues as well as many others.

The Vandenbosch book focuses primarily on South Africa and claims to be an analysis of the foreign policy of apartheid—that is, South African foreign policy since the National Party came to power in 1948. The book is really not very adequate except as an elementary primer on some public aspects of South African foreign policy.

Nearly half the book is given over to the period *before* 1948, for which Vandenbosch does little more than thoroughly review the ideas of General Smuts from his early anti-British/anti-African essay of 1899 (Vendenbosch, however, uses the South African terminology of "Bantu" throughout) to his postelection defeat comments in 1948. In a country with few foreign policy heroes, Vandenbosch is clearly stuck on Smuts, but there is little he can do with the inescapable fact that for all Smuts's high-minded idealism abroad, he was just another racist politician on his own turf.

For the period after 1948, Vandenbosch takes up one issue area after another—the High Commission territories, Rhodesia, the Commonwealth, South West Africa, and the external dimensions of South Africa's internal (eternal?) commitment to apartheid. Here he has drawn largely on United Nations debates and resolutions, in which much of the skirmishing about South Africa has occurred. That pretty much sums up the book. No effort is made to show how any one issue bears on any other. No indication is given of South African foreign policy priorities—regionally, continentally, or worldwide. Nothing is said about how foreign policy is formulated, what interest groups must be served, or for that matter whether South Africa even has much of a foreign policy beyond trying to keep enemies at bay and friends bought off. South African ties to the West—Britain, France, the United States, West Germany—are scarcely mentioned; Japan never is. Until the final pages, South Africa's outward strategy of soliciting friendly relations with black-governed African states is seldom noted, and nowhere is the outward strategy assessed in light of South African commitments to neighboring white states.

Vandenbosch takes a quietly critical view of South Africa, but he sees no point in pushing the whites, instead vaguely preferring to hope for self-generated change from within. If that was not possible during a half century of Smuts, it is even less likely now. The book is surprisingly narrow in scope, given its title, and somehow both tedious and without depth in those issue areas it does take up. There is little here for the specialist and not much more for the general reader.

Potholm and Dale have taken on the broad task of bringing together twenty-three original essays (largely either by academic specialists or governmental spokesman) within a tentative framework suggested by subsystem analysis. Their intention is to link in a single volume the major perspectives on the future evolution of all Southern Africa. The goal was a difficult one to coordinate successfully, and this volume, conceived in 1968, took more than four years to get out.

The length of time between commissioning of the essays and publication has caused the essays to differ considerably in the period they cover; some go only until late 1968, while others take their analysis well into 1971. Needless to say, the twenty-three essays are not all of equal value; indeed, it is all but impossible to compare essays written by academic experts with those commissioned to convey a particular governmental view. Nonetheless, a few essays stand out.

Each of the editors has contributed an essay on the country in which his own work has concentrated (Dale/Botswana and Potholm/Swaziland), and both pieces lucidly and competently lay out the dilemmas that the countries face in their dealings with South Africa. Potholm's essay is set firmly in the framework of subsystem analysis. Dale concentrates on showing us that much about Botswana's politics depends upon the eye of the beholder.

Ronald Chilcote's essay, "Mozambique: The African Nationalist Response to Portuguese Imperialism and Underdevelopment," analyzes the stages of Portuguese imperialism in Africa. Chilcote directs our attention to the deep penetration of the Portuguese economy by Britain. These themes, using this methodology, badly need further exploration for all the countries of Southern Africa. It is really the great weakness of this volume that the nature of foreign investment and trade and the broad penetration of the region by Western powers have not been seriously considered.

Eschel Rhoodie's essay, "Southern Africa: Toward a New Commonwealth?," gives wide exposure to his thesis that because of various ties (largely economic), Southern Africa will develop as a "Third Africa"—a regional grouping as distinct as North Africa and the preponderance of black-governed Africa. John Marcum's essay, "The Exile Condition and Revolutionary Effectiveness: Southern African Liberation Movements," has justifiably drawn widespread attention; it is the most ambitious essay in the collection. Marcum has attempted to analyze the effects of environmental, technical, and existential problems on Southern African revolutionaries in exile. In this richly documented essay, he draws some sobering conclusions on how the exile condition (if not in one way, then in another) generally serves to weaken and discourage revolutionary commitment.

Any number of other scholars have contributed sound essays in their particular fields of interest: e.g., Denis Worrall on Afrikaner nationalism; J. E. Spence on South Africa's outward policy; Douglas Wheeler on Angola; Yash Tandon on the OAU and Southern African liberation. They are solid contributions but generally do not go beyond much of the authors' other published work. Only a couple of the essays are very weak, and they will be readily apparent to any knowledgeable reader. On the whole the essays are well documented and the newspaper footnotes are a tribute to Dale's relentless editing (e.g., "*The Star*/Johannesburg/, July 25, 1968, 2nd city late edition, p. 10, col. 9.")

Potholm's concluding essay, "Toward the Millennium," tries to pull together Southern African trends into what he calls scenarios for the future. Nine such scenarios are postulated. For the short term he sees the status quo prevailing; in ten or fifteen years he suggests an alteration of the Southern African system through the liberation of Angola and Mozambique. Toward the end of the century he suggests the possibility that externally induced change, backed by moderate levels of coercion, will finally reach South Africa. The Dale/Potholm reader brings so much material together that no one with a Southern Africa interest could fail to find it very useful.

LARRY W. BOWMAN
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The United States and Revolutionary Nationalism in Mexico, 1916–1932. By Robert Freeman Smith. (Chicago and London: University of Chicago Press, 1972. Pp. 288. $12.00.)

Two conflicts are represented here: (1) between the rights of the Mexican people to institute revolutionary reforms and the rights of investors in the United States to protect their assets in Mexico, and (2) between the approach of the historian who wrote the book and the needs of the political scientist who reads it.

When Mexico began moving toward greater control of foreign-owned interests, Washington moved to protect the assets of investors from the United States. Professor Smith's chronological description of the events of 1916–1932 is based on a careful search of written material—government documents, private diaries and memoirs—much of which was previously unpublished. More information was obtained from United States sources than from Mexican; south of the border, the author was allowed to see only some of the records in the archives, and reports that "these records were screened before they were delivered to me" (p. 271).

Nevertheless, the book does not manifest the common problem of United States' ethnocentrism. While the picture offered by the author focuses more on the U.S. side, this does not seem to be the result of a biased view. Rather, it follows from the author's interests, the availability of material, and the self-evident fact that the policies of the powerful United States generally had the greater influence on historical events.

The book starts off with background information on Mexico and the revolution. Chapter Two, entitled "The Problems of the 'Backward' Nation," is a good description of the attitude of the United States (and particularly of President Woodrow Wilson) toward what we now call developing countries. Each of the next seven chapters covers a chronological period; the names of some are highly suggestive—"Keeping Mexico Quiet, 1917–1918" (Chap. 5); "Confrontation Diplomacy and the Article 27 Syndrome, 1919–1921" (Chap. 7); "The Temporary Triumph of Finance Diplomacy, 1925–1932" (Chap. 9).

Smith gives us an insight into Washington's foreign policy-making process. We see politicians, lobbyists representing financial and oil interests, and public opinion in action. Mexican decision making is also described, but in my opinion, the portions on the United States are better—perhaps as a natural outcome of the more abundant written sources.

The second conflict mentioned at the beginning of this review—between the historian author and the political scientist reader—is caused by insufficient general comment and analysis. The book is good history, but it would be a better book for political scientists if the author had elaborated on the meaning and the importance of the data. There is no conclusion at the end of each chapter, nor at the end of the book.

Still, Smith does provide some commentary. For example, he thinks that the financial interests fared better than did the oil companies. "Today, Mexican bonds are sold on Wall Street, and the offices of major United States banking firms greet one on almost every corner of Mexican cities. The same cannot be said for the foreign oil companies" (p. 265). In fact, the oil industry was nationalized in 1938, at the peak of an even more dramatic period (Cardenas's presidency); this book provides good historical background for the later events. The author also comments that: "The reluctance to use military force was one of the more important aspects of this competition, and directly influenced the extent and type of control which U.S. leaders were able to exercise in Mexico."

Much of the book is relevant to today's events. The current efforts of the Mexican government to redistribute income are again meeting unfriendly reactions from foreign capitalists who fear what they call "confiscatory policies"; these reactions put serious constraints on President Echeverria's plans for social reform. The book should be read by those who seek progress for Latin America, since it helps provide an understanding of how to deal with the Colossus of the North. It is a guide to the many decision-making forces in the United States Congress—the bureaucracy, mass media, public opinion, and private interests—through which Latin American political action can further the positive aims of revolutionary nationalism.

Although the lack of a summary chapter is a shortcoming, the reader can go back to the preface for comments such as "Analysis of the U.S. confrontation with revolutionary nationalism in Mexico gives, I think, some support to the view that attempts, no matter what the intentions, to control and dominate underdeveloped countries produce more ill will and complications than solutions." The book is an important contribution to the understanding of relations between the United States and Mexico. It also provides a good background for those interested in the political systems of the two countries, and the relations between them.

DAVID SCHERS

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Treaty Sources in Legal and Political Research: Tools, Techniques, and Problems—The Conventional and the New. By Adolf Sprudzs. Institute of Government Research, International Studies, Number 3. (Tucson: The University of Arizona Press, 1971. Pp. 63. $1.50, paper.)

The Institute of Government Research at the University of Arizona has been producing a series of occasional studies in such fields as American government, Arizona government, comparative politics, international relations, and political theory. These have been useful studies that provide us with information and analyses usually not susceptible of being compressed to article length but too limited to warrant extension to book length.

Adolf Sprudzs's review of treaty sources provides a valuable guide to primary materials essential for legal and political studies and, indeed, for studies of certain domestic problems, such as the condition of the environment, that are profoundly affected by the nature and effectiveness of agreements among governments. Fifty-seven pages of text guide the researcher to the vital collections of documents and provide him with indications of the sorts of information that is obtainable from the several sources. Overall, the picture presented is also a guide to deficiencies and, therefore, to the types of work still to be done if researchers are to be able to obtain treaty information with welcome speed and convenience.

Sprudzs properly gives attention to conventional tools and techniques for obtaining and using treaty information. But he also reviews the computer-assisted treaty projects that were in progress at the time of writing. Brief descriptions are provided of the United Nations Treaty Series Project directed by Peter H. Rohn at the University of Washington, the U.S. Department of Defense International Agreements Project, the United Nations Computer-Assisted Treaty Index Project, and the Queen's University Treaty Project under Hugh J. Lawford. The last has sought to establish the most complete records possible concerning British treaties and their application to Canada and other Commonwealth countries.

However, as Sprudzs points out in a brief conclusion, incompleteness of national and international treaty records plagues the researcher. The ideal solution would be the creation of a computerized all-encompassing worldwide treaty-data bank, updated continuously, and dispatching answers to inquirers by teletype. But the probability of states' changing their attitudes and policies is so slim that we must expect to put up with inadequacies that grow more pronounced as the volume and complexity of international agreements grows. To the extent that governments are as hampered as researchers by the inadequacies of outdated information systems, one wonders why we should expect sensible decisions to be made. Yet, as excellent monographs like the one under review serve to remind us of what is needed and what can be done to improve information systems, we can hope that in time information will be more effectively managed.

WESLEY L. GOULD

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Hidden History of the Korean War. By I. F. Stone. (New York: Monthly Review Press, 1969. Second ed. Pp. xvi, 368. $7.50.)

This controversial book, which was originally published two decades ago, has been reprinted, the publishers say in the new introduction, because it serves three purposes: it raises questions about the origins of the Korean war; offers a case study in war propaganda; and shows how the American military and the South Korean government did their best to drag out the war and disrupt the peace talks.

It is appropriate to review this book again, because, as the publishers suggest, some people, particularly anti-war groups, believe or suspect that there is a close parallel between the Vietnam war and the Korean war in all the above three respects. The analogy between the two wars has become more tempting after the disclosure of the Pentagon Papers.

This rather slim volume contains highly explosive arguments and observations. Stone's interpretation of the origins of the war is drastically different from the official version which most political scientists do not question. This well-known journalist attempts to show that John Foster Dulles, General Douglas MacArthur, President Syngman Rhee, and Generalissimo Chiang Kai-shek might have connived to create a Pearl Harbor or a Sarajevo in the Far East in order to "commit the United States more strongly against communism in the Far East" (p. 21).

Mr. Stone speculates, among other things, that North Korea would not have been so foolish as to choose that day and year (June 25, 1950) for invasion even if she had had such a plan, particularly for two reasons: First, at that time the anti-Rhee forces which were willing to negotiate unification were gaining strength, and second, the Soviet Union, which some American leaders believed instigated or inspired North Korea to invade South Korea, was absent in the Security Council where she could have prevented United Nations action against North Korea (p. 65). But a contrary view is equally plausible. On the first point, Stone fails to describe accurately the political situation in South Korea during this period. Contrary to his contention, the anti-Rhee forces held a view similar to Rhee's on the question of Korean reunification. Moreover, there was in my opinion an urgent need for North Korea to invade South Korea because the so-called people's uprisings organized by the South Korean Communist Party had failed, and Communists were ruthlessly being routed by the combined forces of the South Korean army and police. Park Hun-Young, then Deputy Premier and Foreign Minister, according to some sources, urged Kim Il-Sung to save South Korean Communists. Park, who had himself organized and led the South Korean Communist Party (officially called *Namrodang*—South Korean Worker's Party), told the North Korean leader that if the North Korean army invaded South Korea, South Koreans would spontaneously rise against the Rhee government.

On the second point, John Spanier's interpretation is no less plausible or perhaps more convincing. Just as Mr. Stone suggests that the South Koreans with help of the American military maneuvered the Truman administration into a position from which it had no choice but to back their move, so Mr. Spanier hints that the North Koreans maneuvered the Soviet leadership into a similar position (*The Truman-MacArthur Controversy and the Korean War, p. 23*).

From the above speculation on the origins of the war, Stone develops his argument on MacArthur's delay tactics. He argues that MacArthur first hid the information that Chinese communists intervened in the Korean war, fearing that if the fact had been publicized, the American people would have opposed the invasion of North Korea by United Nations forces, and particularly by United States troops (p. 158).

MacArthur, we are told, later exaggerated the extent of Chinese intervention to stall peace moves which seemed to be brewing in Peking and Washington. He purposely risked collision with "Chinese hordes" in the famous Home-by-Christmas offensive because he felt that he would achieve his goal whether the campaign succeeded or not. The author believes that MacArthur's overall strategy was designed not to restore peace in the Far East, but to preserve tension in that area so that the United States would not vacate the region to communism.

The underlying assumption running through Stone's analysis of the MacArthur "conspiracy" is the perfect rationality of the Far Eastern Command. He pictures MacArthur and his aides as cool-headed and shrewd human beings who never made mistakes. Here we find a curious aspect of his reasoning: In his mind Chinese and Russian leaders are vulnerable to all kinds of tricks and deception, while American leaders are not.

This basic assumption runs through his analysis of newspapers and official documents. His analysis of official statements and news reports is truly ingenious and intuitive. One excellent example is his hair-splitting recounting of the number of communist troops in Korea as of December 28, 1951, estimated by the MacArthur Headquarters (pp. 223–24). He tries to show how shrewdly MacArthur exaggerated the threat of a new communist offensive by publicizing the false estimates of enemy strength. He points out that the MacArthur Headquarters contradicted itself by giving vastly different estimates even within a single week. His analysis proves, however, less that the MacArthur propaganda machinery was extremely shrewd and rational than that it was, as I have contended, rather unreliable and inefficient.

Even though his view of the origins of the Korean war is not convincing, it is still stimulating because it is the first major revisionist view of the Korean war. History is full of unanswered questions, and the history of the Korean war is no exception. It is almost inevitable that important historical incidents—and particularly, ideological disputes—are subject to different interpretations. The question of who started a war is as difficult as the question of what is aggression.

In conclusion, the author's journalistic intuition has done more harm than good to his painstaking effort to search the origins of the Korean war.

SANG-SEEK PARK

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A New Isolationism: Threat or Promise? By Robert W. Tucker. (New York: Universe Books, 1972. A Potomac Associates Book. Pp. 127. $2.25, paper.)

Robert Tucker is one of the most reasonable analysts of American foreign policy writing today. His recent book, *The Radical Left and American Foreign Policy,* has been widely read and highly praised because of its thorough understanding and fair rendering of contending arguments in the contemporary foreign policy debate. His new book, *A New Isolationism: Threat or Promise?,* is of comparable quality. The two works neatly complement each other; whereas the former book focuses on explanations of the past, the latter focuses on prescriptions for the future. In both works, Tucker shows himself to be a master craftsman of reasoned discourse; here, at last, is a man with a refreshing concern for valid arguments and a refreshing contempt for silly ones. Yet, as we shall see, *A New Isolationism* is constructed upon an important confusion.

Tucker sensibly begins with a definition of his central term:

> Isolationism is not to be identified with "quitting the world," something we have never done and will never do. It is not to be identified with the absence of all significant relationships but, rather, with the absence of certain relationships. As a policy, isolationism is above all generally characterized by the refusal to enter into alliances and to undertake military interventions (p. 12).

He then asks if the United States can pursue a new isolationism without jeopardy to or sacrifice of its vital interests; his answer is yes, and he proceeds to argue his case with a consideration of "strategic realities," "economic necessities," and "security in the greater-than-physical sense" as well.

In regard to strategic interests, he attacks two familiar arguments which have been used to deny the possibility of a new isolationism: (1) the old argument that a hostile power controlling Western Europe would upset the balance of power and would pose a direct threat to the United States and (2) the newer argument that the U.S. must maintain its alliances in order to prevent nuclear proliferation, because an increase in the number of nuclear powers means an increase in the probability of nuclear attack on the U.S.—the argument that a "nuclear peace is indivisible." He argues that nuclear missiles have made conventional balance-of-power calculations obsolete; "provided that America maintains the strategic forces necessary to deter attack, alliances cannot enhance a physical security that is no longer dependent on what transpires outside the North American continent" (p. 46). He also argues that the prime candidates for new nuclear powers (e.g., Japan, Israel) would be as prudent as the present nuclear powers and also that nuclear weapons have actually made "peace more divisible today than it has been in a very long time" (p. 54).

As for economic interests, Tucker argues that America is not in fact economically dependent on involvement abroad; even if it were, however, foreign nations in their own self-interest would continue trade and investment relations with the U.S., even if the American alliance structure were dismantled. And on the complex issue of "security in the greater-than-physical sense," Tucker offers an appropriately complex argument that takes into account different U.S. interests in the Third World, in Japan, and (by far the hardest case for his position) in Western Europe.

These and other arguments in the book are all put forward with a sureness and subtlety that my summary can hardly capture and in an eminently reasonable style and tone.

No book on such a topic can be free of flaws, however. I, for one, think that Tucker is far too sanguine about the Soviet threat to Western Europe, about the degree to which our lives would be diminished by a Western Europe under either a Soviet hegemony or an anti-Soviet authority, and about the dangers to the U.S. of nuclear proliferation. A foreign policy constructed on the assumption that all new nuclear powers will be prudent at all times is itself not a prudent policy.

Let us stipulate, however, that Tucker has succeeded in demonstrating that there are no good reasons for an interventionist policy. A critic might still ask are there any good reasons for an isolationist one?

Tucker does not give us a systematic list of American foreign policy failures since World War II, which would presumably justify a new policy of isolationism. But if he were to construct such a list, I suspect that it would be rather short. The Vietnamese War is a clear enough case, and perhaps he would add the Korean War, the military interventions in the Dominican Republic and in Lebanon, and the proxy interventions at the Bay of Pigs and in Guatemala. These cases would probably complete the list. (Beyond them, for example, in the economic and military assistance programs, it is by no means clear that the failures are significant enough to justify a new isolationism.)

Here it is important to distinguish between two aspects of Tucker's definition of isolationism, which he tends to fuse or confuse: a refusal to enter into alliances, and a refusal to undertake military interventions. In the perspective of almost thirty years since the end of World War. II, it is doubtful that any of the American alliances has been the cause of a major failure in U.S. foreign policy. None of the U.S. military interventions were required by a U.S. alliance. The much-derided SEATO treaty had to be stretched and contorted to apply to the Vietnamese War, the OAS treaty similarly had to be stretched and contorted to even permit the Dominion intervention, and in the cases of South Korea and Lebanon there was no alliance commitment at all; similar considerations apply to the U.S. proxy interventions. Rather, the American system of alliance treaties has been innocuous at worst and judicious at best. Of course it is true that a military alliance inherently carries the risk of military intervention on behalf of the ally and against external aggression (although not necessarily against internal rebellion), and continued adherence to an alliance could mean war in the future. But Tucker does not argue that this is a likely threat, and he leaves us with no good reasons why the U.S. should withdraw from its present alliances.

In regard to the second aspect of isolationism, a refusal by the U.S. to undertake military interventions within the internal conflicts of other states, Tucker has a strong case. His case would have been even stronger (but much less original) if he had focused his arguments for isolationsim on this one aspect alone.

A New Isolationism is, nevertheless, a careful and reasonable statement of an historically important position. For more than thirty years, American scholars have seen "splendid isolation" as more sordid than splendid. With this book, Robert Tucker has restored to intellectual respectability an ancient and honorable tradition in the study of American foreign policy.

James R. Kurth

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The Rivals: America and Russia Since World War II. By Adam B. Ulam. (New York: The Viking Press, 1971. Pp. 403. $10.95.)

"When it comes to political disasters, Americans are strangely unwilling to accept an error of judgment as an explanation. They tend to seek the answer in moral guilt," the author states on page 93 of this eminently readable book. He also approvingly quotes Napoleon (p. 318): " 'War is often the study of man, rather than men, who is of decisive importance'; the same probably holds true of foreign policies of this age." These two views inform the whole book which is in essence a rather personal and subjective report from the diplomatic and political battlefields of the period since the war.

Ulam's assessment of the postwar relations of the two superpowers is a scathing list of indictments rather evenly divided between the leaders in Washington and Moscow. The "great fault of American policy-makers in the immediate post-war era was *not knowing* [original italics] . . . how strong America was in relation to the rest of the world," and in the subsequent period "the main fault of American policymakers was still to lie in *not understanding* . . . the internal weaknesses and tensions which gripped the communist world and which still offered great opportunities for American diplomacy" (p. 238).

Now, if American leaders are thus accused of grave acts of omission, the Soviet leaders are accused of bungling through acts of commission. Ulam sees much of Soviet postwar diplomacy and political-military activity as seeking the erosion of a feared anti-Soviet capitalist alliance. The Soviet leaders, he suggests, were quite clear about their own military and economic disadvantage vis-à-vis the West, and sought to camouflage this weakness by means of ill-advised militant actions, which only resulted in over-reactions from the West. This curious dialectical vision of recent history, in which the worst of opposing political-military factors clashed sporadically and gave rise to even more terrible syntheses is seen by Ulam as having a dynamic of its own, deriving from the "mad momentum" of nuclear technology and the persistent failure to perceive the adversary's fears and intentions.

The historical terrain covered by Ulam is familiar. There is the drama of Teheran, Yalta and Potsdam; the duplicitous games played by Stalin, who presumably understood the Americans cherished illusions and predilections for moralizing, and above all, the Americans' sense of guilt: guilt over the delay of the Second Front (while the Russian people and soldiers bore the brunt of Nazi armies); guilt over the destructive role in the League of Nations (and thus the feverish efforts to make the United Nations work, at almost any cost). There was also the American obsession to win the war first and only then consider the problems of postwar political settlements; the moral double-standards of the Americans which motivated them to lecture their British allies on the evil of colonies, while conveniently overlooking the creation of a great Stalinist colonial empire in East Europe.

Ulam is rather democratic in his assignment of blame and fatuity in high places. If the Soviets badly desired an American withdrawal from international politics in the postwar period, then they could not have acted more fatuously than they did. There is almost a "design to fail," as perceived by Ulam, in Soviet policy: since the Soviets' great fear was a rearmed Germany, they actually made this terrible nightmare a self-fulfilling prophecy through Stalinist terror in East Europe, the Berlin blockade, and the Korean aggression; if the Soviets desired to keep Communist China in a dependent, "colonial" position, they subsequently destroyed that objective through the Korean war (which enhanced Mao's prestige and self-assurance), the de-Stalinization policies in East Europe, and the doctrinal vendettas in the late 1950s; if they sought to pressure the U.S. to reach a reasonable political-military accommodation in the late 1950s, they sabotaged their own objective because American over-reaction to Soviet strategic threats and diplomatic blackmail resulted in a vast U.S. arms program which clearly established Soviet strategic inferiority.

Ulam stresses "failed expectations" and "misperception" in the relations between the two Rivals as a crucial factor in the deterioration of international politics, and as a key source of pervasive insecurity which led to intensified hostility. The Soviets had anticipated that the U.S. would return to its neo-isolationist position after the war, and instead they faced the Truman Doctrine, the Marshall Plan, NATO, a rearmed Germany and a massive U.S. presence in Europe; on the other hand, the Soviets had expected a strong U.S. interest in Kuomintang China, and instead saw a vacillating U.S. policy which resulted in the victory of the communists in that huge country; having absorbed this "lesson" of China, Stalin anticipated an easy conquest of South Korea by his "proxies," and instead saw the U.S. give its fullest commitment and risk war in that rather politically and militarily insignificant country; and similarly, having learned his "lesson" from the American fiasco in the Bay of Pigs, Khrushchev applied that lesson in 1962, during the Cuban missile crisis, only to realize once more the "unpredictability" of the U.S. leadership.

As for the American side, Ulam blames our anti-Communist hysteria, our moralistic stance toward our allies and enemies, and our technological-economic *hubris* as having heightened the threat-perception of the paranoid Soviet leaders and thus having effectively cost us several opportunities for stabilizing our relations with the Soviets at an early phase of the postwar period.

Ulam's treatment of Soviet-American relations seems to be premised in a particular kind of Realpolitik, leavened with a sense of ethical imperatives: to paraphrase a central theme of his book, the key factor in world politics is power; the Soviet leaders, who are expansion-prone, need to be contained by the application of power; American leaders failed to understand this basic axiom, and missed several opportunities to apply power in order to contain and discipline a weak Soviet Union. This is a rather questionable proposition: one may possibly suggest that it was Soviet weakness and insecurity that propelled the cold war, and as the Soviets became more powerful and more secure, that they also perceived the limits of power in a nuclear setting.

It is difficult to disagree with Ulam's analysis of the Revisionist school of history. He marshals his facts, focuses on crucial timing and sequential events in order to debunk the mea-culpa trance of the ethical chic. If the U.S. went after empire in the postwar period, it was surely in a fit of absent-mindedness, or more properly, without a clear notion of the consequences caused by the simplistic anti-communism of the period. If there was a fatal flaw in our behavior, it was not the imperial urge of American leaders (Truman and Eisenhower are rather unlikely Caesars), but rather the collapse of neo-Wilsonian dreams of F.D.R. and his advisers, the realization that the U.N. was likely to become an unviable institution, and that Stalin and his successors were bent on exploiting the political vacuum in Europe created by the war.

Ulam's treatment of recent history stands apart from most schools and sects of historians. The book will undoubtedly appeal to those who are skeptical of the writings of the revisionists, the universalists, and the ideological sectarians. It will also appeal to those who are disenchanted with the systems-analysis whizz-kiddery, bureaucratic mandarins, and all those quantifiers of politics and history who reduced the total flow of international politics to a new infelicity calculus, to technological banalities dressed-up as political profundities. However, to describe mid-twentieth century international politics in Carlylean or simple Realpolitik terms is rather archaic. For the publication of the book comes at a time when the old order of things, the world of the Two Rivals, is changing greatly. The old verities of the Yalta System no longer hold. The two Rivals are now being challenged by other aspirants to major power, who seek to change the rules of the game, and the two Rivals are finding it necessary to adapt to these challenges. It is not very useful to reduce the global relations of superpowers to a rather simplified game between several political actors who are considered either as dupes or machiavellians. It might be more useful to view these men as being possessed of great power but of dim vision, whose freedom of action is severely circumscribed by the parameters of nuclear technology, institutional-bureaucratic structures, troublesome alliance commitments and the need to maintain the delicate balances of mutual deterrence.

Professor Ulam's book, despite its intensely polemical tone, is an important addition to the literature of diplomatic history and international relations.

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Traité des territoires dépendants. Tome II: L'Oeuvre fonctionelle des Nations unies. By Nicolas Veicopoulos. (Paris: R. Pichon et R. Durand Auzias, 1971. Pp. 501. No price indicated.)

This study is a useful source of information on United Nations activities regarding trusteeship territories since World War II. More than a dozen years ago, Veicopoulos published the first volume, which in 530 pages elucidated the theory underlying the United Nations exercise of trusteeship. Of equal length, this second volume traces the daily concern of all United Nations organs with the trusteeship territories. The goal of the author was to present a handy summary of the trusteeship question in the United Nations. The author has accomplished this aim.

My criticisms are mainly concerned with the methodology underlying the study. It is in the tradition of many European legal treatises, interested in the rules and regulations passed by an agency but without references to the outside world. The trusteeship question in the United Nations is described *in vacuo;* one would have no idea from reading the work that the membership of the United Nations has undergone significant changes in the course of the last quarter-century, that the cold war was raging, that the populations in both the mandate territories and the ruling countries became strongly affected by anti-colonial ideologies.

The granting of independence to the trusteeship territories is generally presented as a victory for the United Nations, the happy culmination of years of resolutions by the Trusteeship Council and other United Nations organs directing the trustee power to lead their charge to independence. In reality this end should be seen more as part of the decolonizing processes of all the empires. The lack of response which South Africa for instance has paid to Trusteeship Council and General Assembly resolutions on South West Africa proves that successful decolonization is due in greater part to the aims and goals of the colonizing power than to the endless United Nations resolutions.

Veicopoulos gives the impression that France granted independence to its part of Togo because of United Nations pressure. But in reality the causes were quite different. The British part had decided to join with the Gold Coast, which was soon to be independent. French Togo, like its British-ruled neighbor, had a mainly Ewé population; the French feared that the Ewé would want to join together the Gold Coast with British Togoland. After ruling their mandate for nearly forty years, the French, it seemed, were destined to see Togo absorbed by the Gold Coast, an English speaking area within the orbit of British influence. Wishing to avoid this possibility, the French decided to give Togo independence, so that it was the African situation, rather than the debates in the United Nations which dictated the manner and timing of independence.

One actual function of the United Nations Veicopoulos does not cover, since he is only interested in the activities inside the glass building in New York; this was the encouragement the United Nations implicitly offered to nascent nationalist movements in the mandate territories. Knowing that there was an international body to which they could send their grievances and from time to time visiting missions to which they could present their views on the development of the territory, these movements gained a sense of importance. Also, the knowledge that the ruling power was responsible to an international body probably reduced respect for it and thus fostered the growth of nationalist movements. But these considerations are unfortunately outside Veicopoulos's purview.

If there were forces in the mandate territories affecting the administering power, there were also domestic forces in the latter shaping the trusteeship relationship. In the early 1960s the United States, Veicopoulos shows, was more closely and sympathetically listening to suggestions regarding its rule of mandate territories in the Pacific. Some of the idealism of the John F. Kennedy administration (or at least its desire to appear idealistic) and its desire to curry favor with the newly independent, recently colonized nations explain the liberalization of United States trusteeship policy, rather than, again, any particular accumulation of United Nations resolutions.

Veicopoulos uncritically accepts the United Nations goal of independence for all mandate territories. He does not seem to find strange, for example, the United Nations insistence on total independence for the tiny (less than ten square-mile) coral island of Nauru. In 1968 the 3500 inhabitants were given independence by Australia. As former Secretary General U Thant, certainly no friend of colonialism, pointed out, sovereignty might be too heavy a burden for small areas to carry. One might well ask how Nauru and for that matter a large number of other ex-mandate territories really can be independent. The former rulers still retain enormous influence.

Traité des territoires is solely based on United Nations documents; Veicopoulos depends on these sources so much that he even cites trusteeship documents for geographic information on the location of various territories. There are no interviews with former participants, and no study of the world press in order, for instance, to find out what the underlying motives were for some of the speeches and resolutions made on the trusteeship question. If a document was not published by the United Nations, it is not of any interest to the author.

While the book is a useful summary of United Nations resolutions, and decisions, it fails to give the larger context within which they occurred, or realistically to assess their impact. Admittedly, these were not the goals of the author. But lesser aims lead to little more than a dry summary of resolutions on trusteeship territories passed by various United Nations organs. One is left wondering what they meant.

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British Foreign Policy in the Second World War. Volume I. By Sir E. L. Woodward. (London: H.M.S.O., 1970. Pp. 640. £3.62½ p.)

The late Sir E. L. Woodward's long and detailed history of British Foreign Policy in the Second World War was originally written between 1942 and 1950 for official use, and revised thereafter. For a long time, the appearance of the full version was prevented; although an edition in one volume, which must have cost him an infinity of labor to compress, appeared in 1962. It is hard to understand why so prolonged a delay should have occurred, but it has at least two advantages: the author has been able to make use of many sources which have appeared in recent years, and now that the Archives of the British Government are open up to the summer of 1945, it has been possible to equip this edition with references to individual documents so that they may be speedily traced in the Public Record Office.

This volume consists largely of narrative. Sir Llewellyn Woodward's desire has been to provide material for judgment rather than to analyse or philosophise at length. He was not instructed to defend any policy or person, but, in paying tribute to Sir Winston's six volumes, observed that the latter had "rightly allowed himself a personal approach and a freedom of comment from which an official historian is debarred" (p. v). Sir Llewellyn has tried, and with considerable success, to avoid the besetting sin of books on foreign policy, that of isolating a particular aspect and then concentrating upon it to the exclusion of almost all else. As Mr. Eden once remarked "there are no chapters in foreign policy; only chapters in books about it." Hindsight is carefully differentiated from information available when decisions were made; and a balance has been maintained between the isolation of particular themes, absolutely necessary unless a book on so large a subject is to be a jumble, and their interactions. Statesmen, we are reminded, have to make their choices amidst a welter of business, often with incomplete information, and never more so than in the period covered by this volume. No re-creation, however, skillful, can convey adequately the manner in which different forces and crises converge or the full play of the personalities; nor will the historian imagine that free access to huge archives will give "the whole story." Sir Llewellyn claims nothing of the kind. He acknowledges that much will be learned from archives and private collections not yet open. He remarks at one point that the minutes of the Permanent Head of the Foreign Office during this period, Sir Alexander Cadogan, are delusive in their quietness and modesty. The same judgment might well be applied to Sir Llewellyn's own work, to which he brought acquaintance with most of the leading personalities of the day, unique knowledge of the archives, lucidity, balance and fair-mindedness. His judgments, though infrequently interposed into the narrative, lack nothing in sharpness; there is, for example, a timely onslaught on the prevailing orthodoxy about unconditional surrender (Introduction, pp. liv-lv).

The introduction, a magisterial survey of the broad issues in British foreign policy during the Second World War and of the conditions in which it had to be conducted, all pressed into a little less than 40 pages, should be studied by anyone who picks up a volume of this series. Because there is comparatively little attempt at the evocation of atmosphere or description of character, what is written on those scores is the more effective. There is a mastery of understatement; a most hearty and unfeigned detestation of Nazi policy and ideology; a welcome insistence that it is no part of a historian's duty merely to poke fun at statesmen or deride their mistakes.

Sir Llewellyn explains how the Foreign Office was able to sustain its position far better in the Second World War than in the First. In the phase covered by this volume the formal functions of the Foreign Office were of course much reduced. Sir Edward Grey used to say "don't rely on your foreign policy to protect this country," but successive Foreign Secretaries between the Wars, and especially after 1935, had been compelled to conduct a foreign policy without the means to make it effective.

"In wartime," Eden wrote, "diplomacy is strategy's twin." This volume therefore needs to be read in conjunction with those in the *Grand Strategy* series and with others published after the War, and especially with Professor Medlicott's *The Economic Blockade*. Sir Llewellyn interviewed many of the leading actors on the British side and took their comments on his draft into account; he had available not only the papers of the Foreign Office and of the War Cabinet, which he might have used more freely had he foreseen their early release, but also many of Mr. Churchill's private papers, including the correspondence with Stalin and Roosevelt, and the records of the principal conferences.

The events which this book records were of the most cataclysmic kind. The twilight war; the Russian onslaught upon Finland; the fumblings and hesitations of the allies over Scandinavia; the balancings of Mussolini; the collaboration between Russia and Germany; the collapse of France, and with it of the assumption upon which every British Government since the First World War had based its European strategy, namely that the French Army would provide the main bulwark against a resurgent Germany; and the abortive offer of an Anglo-French union followed by the recognition of General de Gaulle—all these are recounted in a dry, unemotional, almost matter-of-fact way.

For Britain, survival rested upon the ability to prevent Japan from breaking loose in the Far East, even at the price of closing the Burma Road for three months in the summer of 1940; upon the ability to secure material help from the United States, though it came late and in lesser quantity than Mr. Churchill had hoped; upon preserving the neutrality of Spain; and upon playing a waiting game with Russia. Mr. Molotov's contemptuous treatment of Sir Stafford Cripps is well described. Russia supplied raw materials and openly proclaimed the fact, in order to enable Germany to evade the consequences of the British blockade. Britain could do little therefore but keep Germany at bay, hold out against air attack, maintain mastery of the Channel and the North Sea, and take the risk of diverting substantial forces to the Middle East, the one theater in which she might be able to strike a serious blow on land against the Axis. Not until 1943 did that campaign come to its successful conclusion, after many vicissitudes; whereas this volume concludes in 1941 with British warnings to Russia of impending attack and Mr. Churchill's broadcast offer of assistance, made because of his conviction that the invasion of Russia was a prelude to the invasion of Great Britain and therefore that the Russian danger was "our danger and the danger of the United States."

Let us hope that Sir Llewellyn Woodward's narrative will soon be supplemented by volumes of documents drawing not only upon the archives of the Foreign Office but also upon the papers of the Cabinet, the Ministry of Economic Warfare and the Service departments. Meanwhile, this book represents marvelous value at £3.62½ p.

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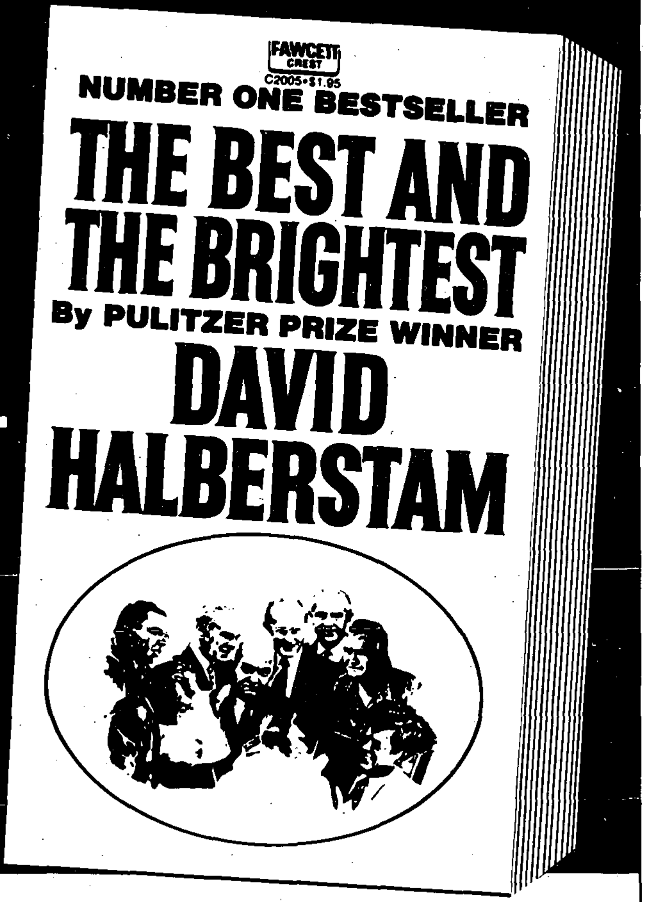
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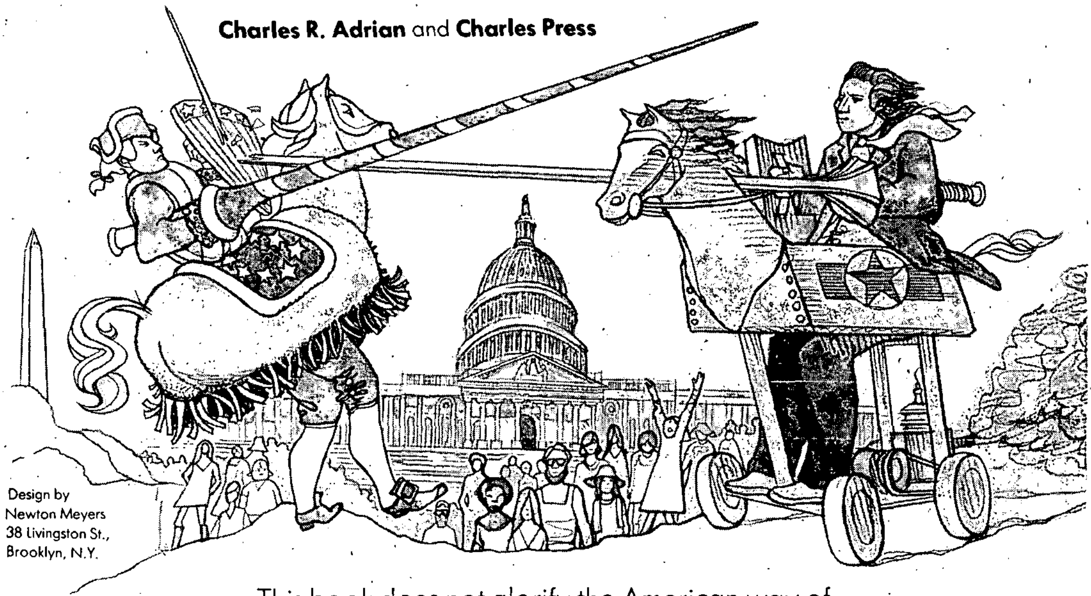

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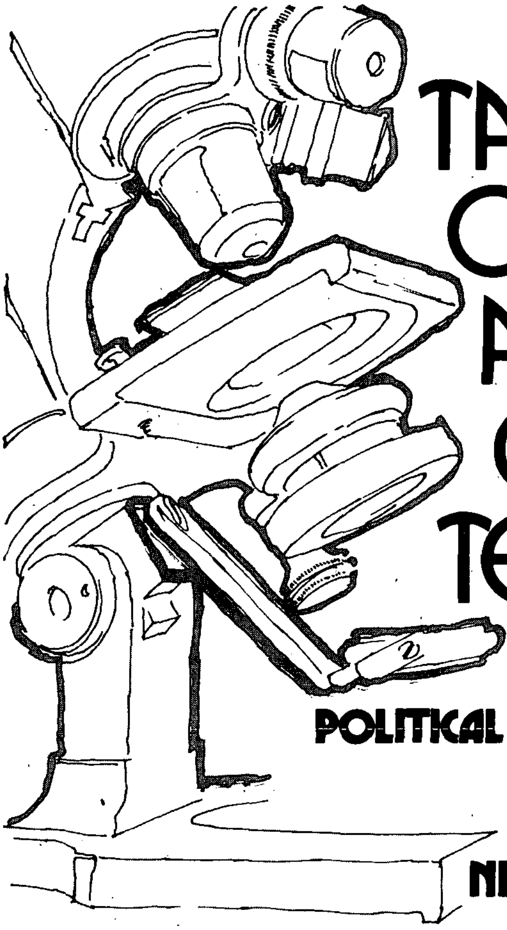

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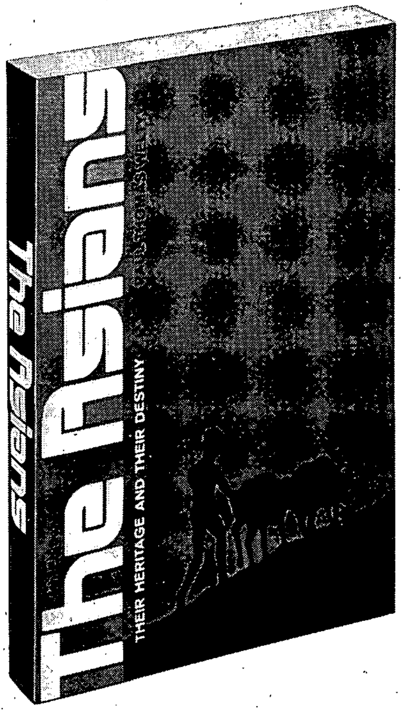

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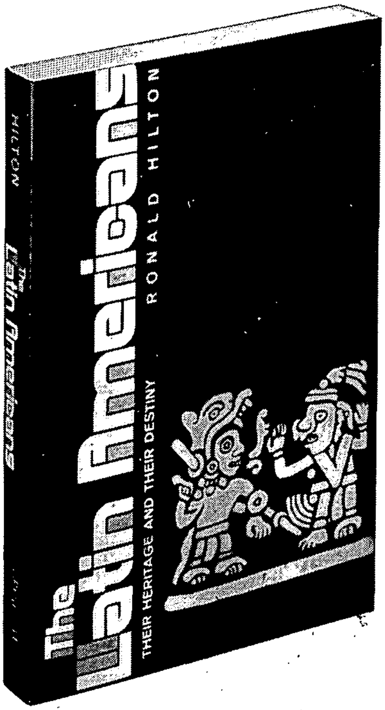
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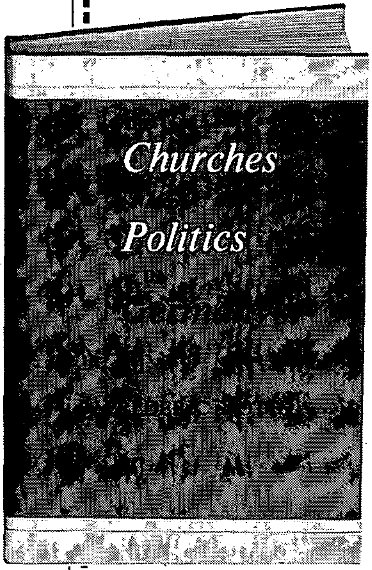

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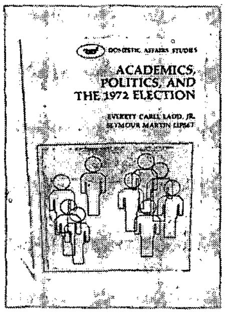

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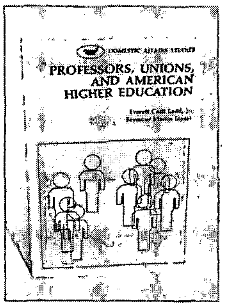

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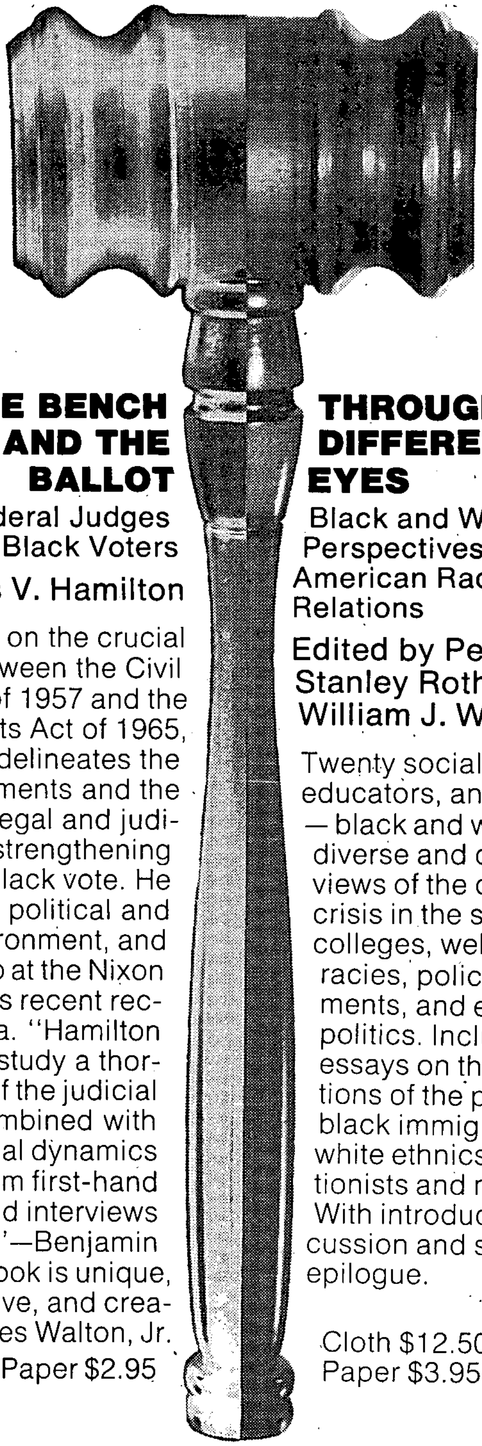

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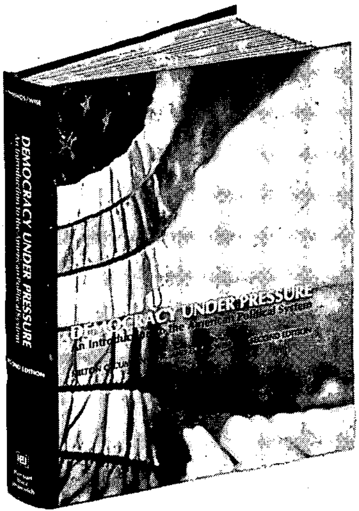

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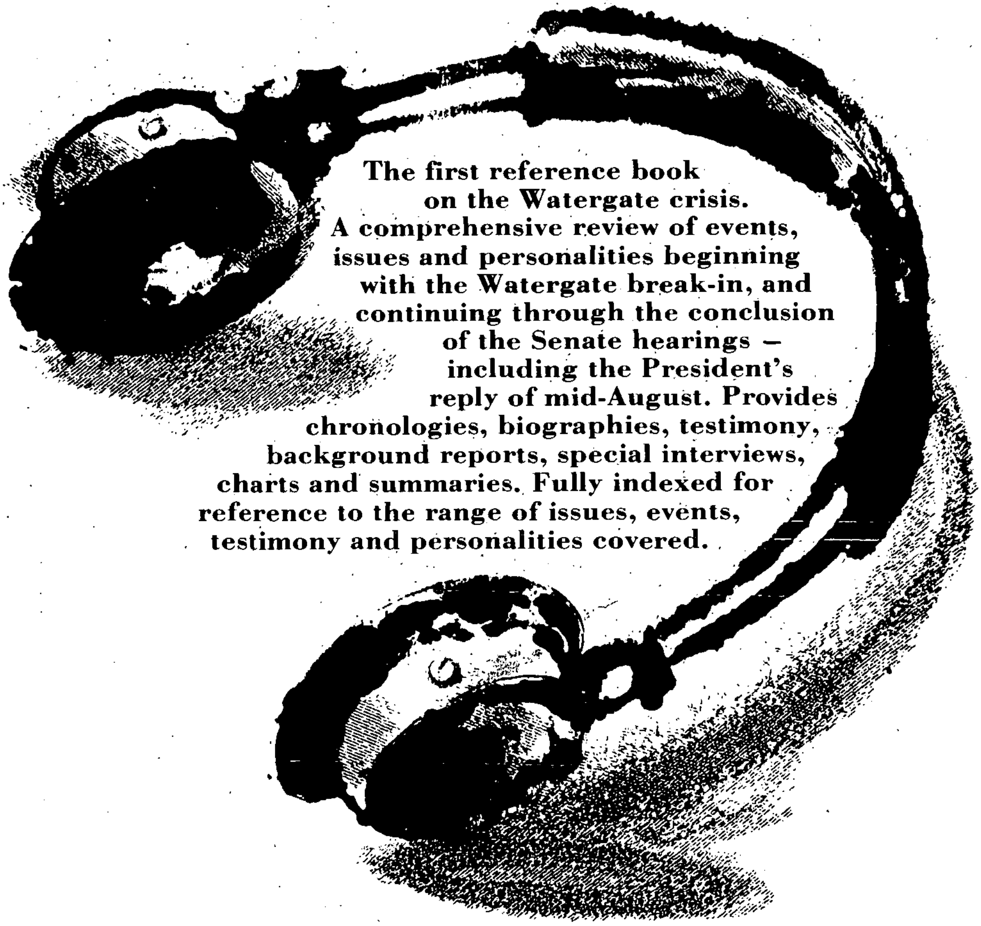

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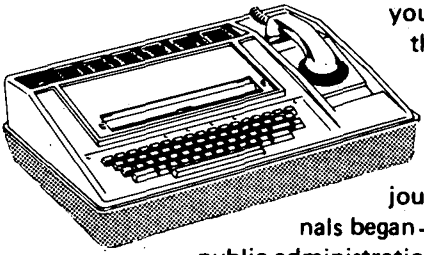

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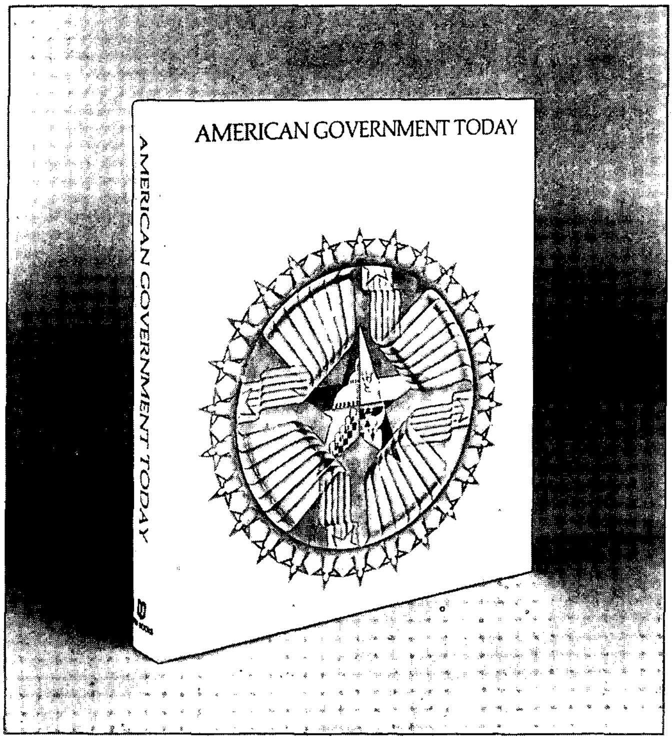

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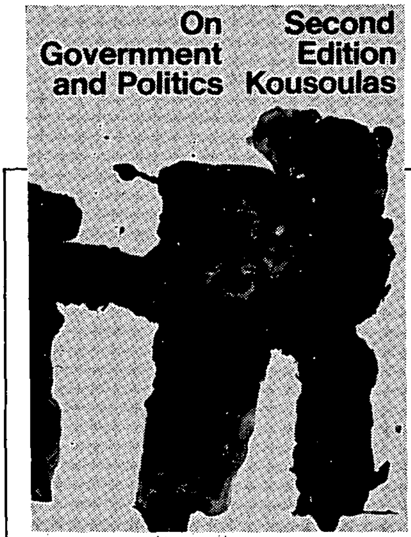

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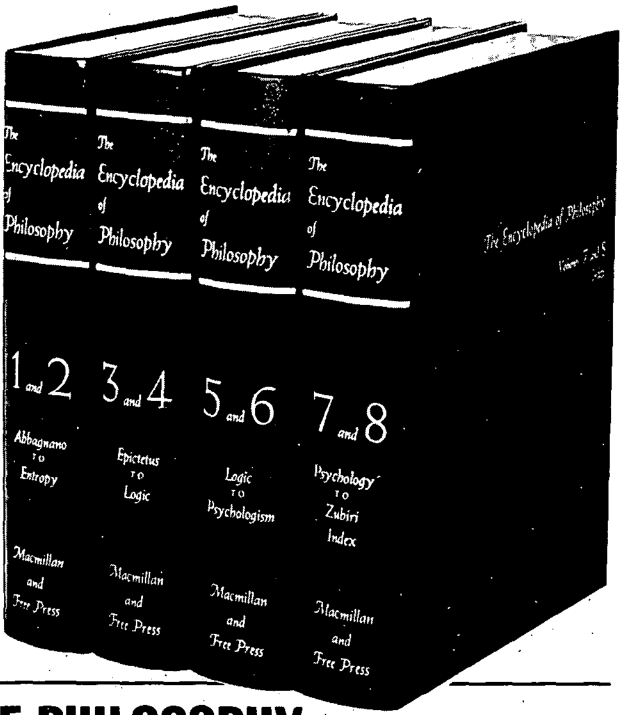
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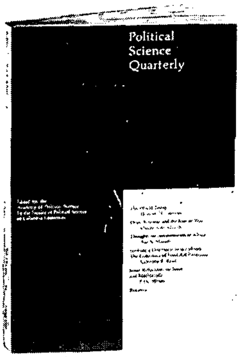

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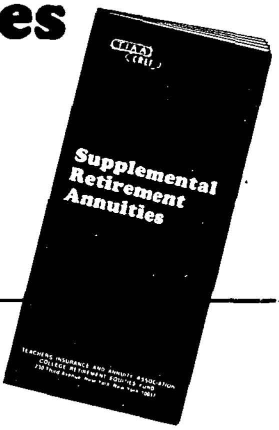

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